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AMERICAN ACADEMY OF DENTAL SCIENCE.

THE regular monthly meeting of the American Academy of Dental Science was held at Young's Hotel, Boston, January 6, at six o'clock P.M., President Andrews in the chair. The paper for the evening was read by Dr. J. L. Hildreth, of Cambridge, Mass. Subject: "To what Extent is Typhoid Fever Preventable?"

President Andrews.—Fellows of the Academy, it was my privilege last evening to attend the meeting of the New York Institute of Stomatology. Many of its members are also members of the Academy. Among them I met Drs. Benj. Lord, Geo. S. Allan, and E. A. Bogue, who wished me to remember them to the fellows of the Academy.

I have the honor to-night to present to the Academy Dr. John L. Hildreth, of Cambridge, who will read the paper for the evening.

TO WHAT EXTENT IS TYPHOID FEVER PREVENTABLE?

BY J. L. HILDRETH, M.D., CAMBRIDGE, MASS.

Not long ago I read something that interested me very much with reference to the practice of law in London, in which a distinguished judge said that if one of his predecessors of a hundred years ago was to come back and sit upon the bench, he would find it just as easy as when he sat there before. I thought to myself that we were rather proud that we could not make any such boast about medicine. The man who would attempt to practise medicine this year as he practised it twenty-five years ago would certainly not maintain his hold upon the community, nor do as good work as those around him, and he would be looking for work elsewhere. The contrast is very forcibly brought out in this comparison be-

tween medicine and law; the practice of medicine is progressive, advancing from day to day, while the law probably remains as it did one hundred years ago.

I suppose it would be fair to say that this progress, which medicine has made, has been no more signal in any direction, and no more important in its results than in regard to our knowledge of the cause and the successful treatment of diphtheria. It certainly stands foremost in the work which the profession has done for humanity. Next to that, I think we may place typhoid fever. We cannot take the same forward step with regard to the treatment of typhoid fever as with diphtheria, and prescribe and use the antitoxin, but I think it is fair to say, with reference to typhoid fever, that we know quite as much about the cause and the course of the disease, the way in which it gets its hold upon the system, and the effect which it has on the patient,—we know a great deal more about it than we do with reference to diphtheria; and yet, when your committee asked what should be put on the card, I purposely put it in the form of a question, because I do not know that I, or any other medical man, could answer the question as to how far typhoid fever is a preventable disease.

I simply thought that I would state to you the facts with reference to typhoid fever as they are known to the profession to-day. Of course, I cannot expect that these facts will interest you as they do us who are in the harness of every-day work and seeing it everywhere, but they have great interest to the layman, as I think I may demonstrate to you, and after I have stated these facts, then I will let you, each one for himself, answer the question if he may, as to how far it is a preventable disease.

I know that in every-day life things that are very familiar, that are all about us, that are common with us, do not impress us as those things that we but rarely see. I remember, in the last epidemic of small-pox, that Boston was crazy over thirty or forty cases, while the fact that one-seventh of all the deaths that occurred in the city was caused by consumption awakened no thought or interest among the people, although the number of deaths was a hundred-fold more from consumption than from small-pox. There is that same lack of interest when we speak of a middle-aged person having typhoid fever, because we see so much of it and have seen so much of it all our lives, but when you consider the great amount of typhoid fever that occurs every autumn, from August to November, and when you consider the length of sickness of each individual case, if the case runs a successful course, together

with the term of disability which follows after the disappearance of the disease, and when you consider the amount that has been expended in the payment of the bill for nursing and the doctor's bill for a single case, and multiply that by the total number of cases, you will be surprised at the enormous amount which typhoid fever alone costs the people of Massachusetts. Take, for instance, our little Cambridge Hospital, which has had an unusually large number of cases the past six months, July to January, and where we have but thirty-four beds; we have entered eighty cases, having about fifty there at one time which we were obliged to do our best to care for on account of the great number that demanded admission. Now, if you stop to think that each case costs the hospital for the care and treatment (making no account of the cost of the plant, which originally cost something like one hundred thousand dollars, besides the equipment) about thirteen dollars; then if you remember that most of these cases that get into the hospital will certainly average as much as six weeks; then multiply six by thirteen and that by eighty, and you will see that you have a sum a little above six thousand dollars, which it must have cost our Cambridge Hospital this fall for typhoid fever alone. The cost to those persons who are able to pay for their treatment is no trifling amount, the expense of a nurse for three or four weeks and the attendance of a physician and the other incidental expenses. I think your president can well remember his first year in Cambridge when he had typhoid fever, and that the expense of it was quite a large sum. You know that a person is not only disabled in earning capacity during the continuance of the disease, but, many times, for two or three months afterwards; so that the expense of typhoid fever in Massachusetts, if we knew how much it cost to take care of the sick ones, and added to that the loss of wages,—which means a great deal to poor families, and even to those who are well-to-do,—would seem almost incredible. This is not all. The death-rate during most epidemics is rarely less than ten per cent., and oftentimes comes up to twelve, thirteen, and even fifteen per cent., so that the distress is great in addition to the loss.

There have been some very interesting epidemics of typhoid fever, and I will refer to the more remarkable, some of the earlier ones which attracted attention and which started the investigations which brought us to where we are now in our knowledge with reference to the development and extension of the disease. The investigation of one of these epidemics showed the facts to be that a man was going home from Vermont, and passed the night in a

little village near the eastern border of New York. He was then suffering from the beginning of an attack of typhoid fever, the earlier symptoms of weakness, diarrhœa, etc., and through his diarrhœal discharges he contaminated a well which was used in the village where he passed the night, and in that village, the population of which numbered nine families, a total of forty-three persons, one-half of them contracted the fever and ten of them died. You will remember, the most of you, the great number of sick people who came from Philadelphia at the time of the Centennial in 1876, and were found to have typhoid fever. Nobody will ever know how many cases or deaths from typhoid fever could be traced to that exposition, or what was the cause of the epidemic, although it is very probable that the drinking-water was contaminated. A great many of these epidemics have been traced to the contamination of drinking-water, and the English people always speak of such epidemics as "water-borne" typhoid. One of the most interesting of these water-borne epidemics occurred in 1877 along the banks of the Ohio River. The water in the river became contaminated with the excreta from some case or cases, and the whole length of the river, a distance of some eight hundred miles, it was one epidemic of typhoid fever. Then in the town of Plymouth, Pennsylvania, in the year 1886, there was a contamination of a water-supply which was used by about eight thousand of the inhabitants. There were one thousand cases of typhoid fever in that town at about the same time, of which one hundred died, a mortality of ten per cent., and of those who contracted the fever, seventy-five or eighty cases were taken sick on precisely the same day.

You know, of course, that the number of cases of typhoid which were contracted at Chicago, during the World's Fair, was very large, but it was undoubtedly very much lessened by the precaution which they took to introduce Waukesha water, and a very few people, comparatively, used the water of the lake. I suppose if you should go into the larger cities, you would find a great many people who came home sick from the fever, contracted in Chicago,—I know that is certainly true of Cambridge,—but the number was not so large as it certainly would have been if they had not taken the precautions they did.

With reference to armies, death by typhoid fever often reduces the ranks as much as the enemies' bullets. In the Civil War the number who died from typhoid was enormous. I tried once to get the statistics, but they wrote me from Washington that they had never been published. The statistics of the French army, for

the period from 1875 to 1891, estimate the number of cases of typhoid fever to have been one hundred and thirty thousand, of which ten to fifteen per cent. died. You know that we had in Lowell a few years ago an epidemic which was caused by the excreta from three persons contaminating the water there, the Merrimac, at Chelmsford. I was asked to go there and see some of the cases; there was a difference of opinion as to whether they were really typhoid or not, but when I went into one of the wards and saw fifteen or sixteen cases in a room it reminded me of the hospital tents during the war-times. At about the same time thirty or forty cases appeared in Lawrence, and an investigation showed that the epidemic along the river was undoubtedly due to the contamination of the water by the excreta from those three original cases.

These epidemics of typhoid fever from contaminated water-supply have been known not only here, but have also been recognized in England. One of the most notable outbreaks was that which was known as the "Blue Hill epidemic," in which some of the excreta from two cases was cast out on the side of a hill in the winter time, and there came a rain and a melting of the snow, and this excreta was taken into the water and carried down to a town thirty or forty miles below, where the water was used for drinking purposes, and a very large number of people contracted the disease.

Later investigations have proved that typhoid can be conveyed by the use of milk, so that we now have milk epidemics as well as water-borne ones. One of the earliest of these milk epidemics took place in Cambridge, and for the successful investigation and the undoubted proof of its origin the Board of Health in Massachusetts has great praise. Since that time their expert, Professor Sedgwick, has examined twenty-eight epidemics which he has traced to the contamination of milk. The large number of cases which we had in Cambridge the past year was almost entirely due to milk. We have had three milk epidemics there in one year. I trust this may not influence any one from going there to live and making use of the educational advantages which we have to bestow, and appreciating the beauty and convenience of the city as a place of residence,—but we have been unfortunate this year. In one of the epidemics seventy cases came down in ten days, and of these, sixty of them occurred in the route of one man. The proprietor of the milk-route was a very fine man, and he had no idea that the milk which was being delivered to his customers was carrying sickness and death to so many of them.

Another source of conveying the disease is oysters. As we sat at dinner I conversed with my friend from Providence, and I thought of the Middletown epidemic which they were unable to account for until they found that a great many of those who had been taken with the fever had eaten oysters which were grown in a portion of the river close to which there had previously been a couple of cases of typhoid fever, and it was learned that the dejectures were thrown into the river. English people have done more than we have in working up oyster epidemics. Fortunately, they are rare, but oysters must not be overlooked in the investigation of an epidemic.

Now, these sources of conveying the disease to patients are the ways that we know most about, but I fancy, as time goes on, many other ways by which the disease is conveyed will become known. But we have been convinced of one thing: that the germs of typhoid fever enter the body through the intestinal track, and probably in no other way. Our investigations have led us to the belief that no exhalations from any dirty source, no offensive smells or putrefying substances, will cause typhoid fever; that the drinking-water may be bad, vile in every way, smell, taste, and appearance, but unless it has present in it the germ of the disease, the typhoid bacillus, it will not produce typhoid fever. Now, this is the result of the later studies and knowledge that have been acquired in reference to this disease. We know that oysters and milk and water convey typhoid fever, but we do not know that it may not be conveyed in other ways. If you should take the statistics of a hospital and go over the typhoid fever cases and see, if it were possible, how many were water and milk cases you would find a great number which must have been contracted from some other source. Now, if you stop to think, you will see that those things that I have alluded to—milk, water, and oysters—are about the only food which we take into the system that is uncooked,—aside from fruit, as for instance, the banana, the apple, and the orange. These are not usually cooked, and it is possible may be sources of conveying typhoid fever and other diseases as well. Also, there is another reason, which is that milk and water are believed to be the media in which the typhoid germs grow with the greatest rapidity; they are the best media for their culture, except some artificial media which are made from bouillon or something of that sort. Pettenkofer, who was really an extraordinary man, and who spent a great deal of time in the investigation of fevers, and Louis, another man who took a deep interest in the study of fevers, believed in the

theory that typhoid fever originated from the ground in some way and that it might float in the atmosphere, and thus cause epidemics at certain times and places. We know of nothing to prove that emanation from the ground or from any unclean or decaying substance will produce typhoid fever, although I do not see why the germ should not occasionally get into the intestinal tracts through the air-passages.

Gerhard, of Philadelphia, in 1834, demonstrated that typhoid fever and typhus fever were distinct diseases. Before that time they were regarded as the same disease, although they knew that some cases presented ulcers in the intestines, while others did not, and these they called abdominal typhus. On the other side, Sir William Jenner, who gave us so much work on vaccination, published, in 1849, a valuable paper, in which he stated that the two diseases were then conceded to be quite different. Typhoid fever is due to lesions in the intestines and digestive organs, and in Peyer's patches, and the glands which are carrying on the process of digestion. Typhus fever is essentially a disease of the blood. We do not know very much about the cause of typhus fever,—that is, what germ is present in the disease,—but we know that it seems to find a very desirable habitation in jails and houses where men and women are crowded, and where they give little attention to cleanliness. It was this disease which was present in almost all of the ships of England in Howard's time, and was looked upon as a dreadful plague, as it certainly was.

The bacillus which is present in typhoid fever is very much like another bacillus which is present in the intestinal tract in health, not only in man, but in many animals. It is present in the cow and the horse, but, I believe, it is not present in the dog. It is called the *coli communis*. That bacillus when viewed under the microscope without any particular stain seems very much like the bacillus of typhoid, but it grows in a different media, and looks quite different when it is subjected to certain stains. Under the microscope these bacilli may be seen very readily with a twelfth immersion, but not with a power much below that. These photographs that I show you are simply to show the different shape and form and different lengths which they manifest when they are presented under the microscope. They are copied from a photograph by Sternberg, who has done a great deal of work in bacteriology, and are quite true to nature. The ordinary bacillus will average perhaps a twentieth of an inch in length when it is magnified one thousand diameters. As I have told you, the bacillus of typhoid

looks very much like the *coli communis* which is present in nearly all animals, and is the commonest thing in the intestinal tract. Just here let me say something which may interest you and which is of great interest in the investigation of typhoid epidemics. Where you are looking for contaminated drinking-water, the present method is not simply by making chemical tests, not by taking a specimen of the water and placing it in a cone-shaped filter and examining the sediment under the microscope, but by making cultures from this sediment and growing them in various kinds of culture media, and staining them until the bacilli can be completely identified. I have no doubt that a great many mistakes were made by those who, years ago in examining drinking-water, found this bacillus which they thought the typhoid, but which was only the *coli communis*. It is not unlikely that many times the water examined contained the excreta from some horse or cow or other animal, and not understanding the difficulty of separating the *coli communis* from the typhoid bacillus, no doubt many mistakes were made,—that is to say, you cannot take the bacillus and stain it with one stain and put it under the microscope and say that it is the typhoid bacillus; you must go through quite a process of staining before you can be sure of the identity of the bacillus.

The discovery of this germ (although the possibility of its existence had been suspected by quite a number of workers) was made by Eberth, who discovered it and announced it in 1888, so it is less than ten years that we have known positively that there was a typhoid bacillus, and that in no case does the disease exist without it.

This is where we stand at present in our knowledge of this disease,—that the typhoid bacillus *must* be present; that it probably enters the body in no other way than through the intestinal tract, and there finds a medium which is agreeable and conducive to its propagation, and in some way gets into the glands which we call Peyer's patches, which secrete the material used in digestion, and also in the spleen. The spleen is a particularly favorable place for their growth, and it is said that you can get almost a pure culture of the typhoid bacillus from the spleen of a person who has died with typhoid. Now, while these parts are the most favorable for the reproduction of the bacillus, and are what may be called its home, yet if it occasionally gets into the circulation,—by that I mean into the blood,—and is carried about and locates itself in other parts of the body, then we have what is called a typhoid septicæmia, a general poisoning of the body. These

cases of typhoid take on very much the same form, and the person affected has much the same temperature-chart as one who is suffering from galloping consumption or miliary tuberculosis. Now, this is about all that we actually know with reference to the typhoid bacillus. We know nothing of its life outside of the body, or where it lives, or what it does. A good many laboratory experiments have been made with reference to its life in water, and it has been known to live for weeks and sometimes for months in water about the temperature of the body. Pruden froze the typhoid bacillus in a block of ice and kept it there for months, and when it was thawed out, he found the bacilli as lively and active and ready for work as if fresh from a patient. It is a very lively bacillus, always moving, kicking, and working about in all directions. No one has ever been able to take a bacillus and cultivate it and see it with anything below the twelfth immersion under the microscope, and even then it moves so rapidly in every direction that it is almost impossible to tell what it looks like. Of course, if you stain it you destroy its activity.

It is probable that this bacillus has a life outside of the body where it lives and propagates, and that we simply see it in its wanderings when it gets into the human body and causes so much trouble. What is still more remarkable is the fact that it is not the bacillus itself that does the harm. Its behavior and its life in the human body are precisely the same as with diphtheria bacillus, except that it does not produce its effect so quickly. The bacillus of diphtheria, although it is such a dreadful thing, does not of itself produce much trouble. It is the toxin which it generates and which is absorbed into the system that causes all the trouble. And the same is true of the typhoid bacillus: it is the toxin which it produces that causes the disease we call typhoid.

Now, right here I might say that it differs from diphtheria in one respect, and it is because of this difference that our hope of the future is very much less than with diphtheria. As I have before stated, the bacillus of diphtheria produces a toxin, and it is that toxin that causes the sickness and death of the patient. Now, we have learned that as soon as that toxin is present in the system of the patient nature at once goes to work to manufacture an antitoxin which counteracts the effect of the toxin, and the patient's life is saved in that way. It has also been discovered, and I may say that it is one of the most important discoveries ever made by the medical profession, that we can produce diphtheria in the horse, and as the system of the horse manufactures the antitoxin,

we may extract the horse's blood, and by injecting the serum into the diphtheritic patients assist them in overcoming the effect of the toxin and making the system an uncongenial habitation for the bacillus.

Unfortunately, typhoid fever cannot be produced in any of the lower animals. Very many experiments have been made in trying to produce the disease by inoculating horses, dogs, cats, rabbits, but without successful results. If we could produce typhoid fever in the lower animals, then we would go to work and extract the serum which they have made to enable them to throw off the disease and inject it into typhoid patients and thereby hasten the immunizing process which goes on in each individual, and so save many lives, just as we have been able to do in the case of diphtheria; but no animal has yet been found that can have the typhoid fever, and so far we have no hope of making an antitoxin which we can inject into the bodies of persons suffering from typhoid fever, and by serum therapy assist them in overcoming the disease.

It is a very curious fact with reference to the bacilli of diphtheria that while we understand more about them, we do not know how soon after being taken into the mouth they begin to manufacture their toxin, but we know that they work very rapidly, and begin to make their presence felt perhaps within a day. In typhoid fever, so many observations have been made that we know almost to a certainty that a person who drinks water which is contaminated by the typhoid germ—if they are susceptible to the disease—will be taken sick by the fourteenth day, so that, in case of a milk epidemic, for instance, if a person has taken contaminated milk, and passes by the sixteenth, or seventeenth, or eighteenth day, you may feel pretty sure he will not come down with the disease.

Before the introduction of the antitoxin treatment for diphtheria we used to say, when we were doing our best to combat the bacillus of diphtheria, that if the patient passed the fourth day he stood some chance; if he passed the sixth day, he stood a greater chance, and after the eighth day he was pretty sure to get well. With regard to typhoid fever, we now say that the twenty-first day usually marks the crisis. I have been looking over the records of some of the cases in our hospitals, and I have been surprised to see how closely this rule is followed. In most of the cases the patient began to get normal after the twenty-first day, unless there was a relapse, a return of the disease, showing that the system had not manufactured antitoxin enough to control the disease. I had a good opportunity to observe this regularity in the time of the

last epidemic in Cambridge and the epidemic in Somerville a few years ago. In tracing the route of the milkman who supplied the milk, it was learned that every one of the patients came down on the same day, almost precisely,—I think there were thirty-three cases that came down within one or two days. In that epidemic—the Somerville one—the young man who had the distribution of the milk, the milkman's son, had typhoid fever, and no doubt he was uncleanly in his habits and some of the dejections from the bowels got into the milk, which was a beautiful place for the germs to live, and consequently the disease was communicated to those who drank the milk. The Cambridge epidemic was due to a young fellow who had what we call "walking typhoid,"—that is, while he felt sick, he continued to work through it all, not being sick enough to require him to leave his work. When these outbreaks occurred, you could take a map and, by following the routes of these milkmen, mark down the limits of the epidemic. Of course, not all the people on these routes were taken down, because it is rarely, after forty, that a person contracts the disease, and, besides, some persons are not susceptible to the disease, just as you know some persons are not susceptible to small-pox or any of the ordinary contagious diseases.

There has been an attempt made to treat typhoid fever in the same way that Pasteur undertook to treat hydrophobia, which, as you know, was by taking the essence of the disease, the spinal cord, and making cultures from it, and injecting that, in dilute solutions at first and gradually increasing the quantities, immunizing them more and more until they became so that they were not susceptible to the disease. That was his method of treating rabies, and to some extent he thought he was successful. A method something similar to this was tried in typhoid fever last year, in the hope of cutting short the disease, with some good results; but it did not commend itself to the profession, and it has not been carried on this year.

It was early known that typhoid fever was contracted a great deal in the country, and that it was caused by contaminated wells; and even before the presence of the germ was fully understood, the physicians in Boston made prophecies that if the water-supply of Boston was free from contamination, typhoid fever would disappear from that city. Dr. Henry I. Bowditch made that prophecy many years ago. In cities and towns where they are very particular with reference to their water-supply, and especially in those places where great care is also taken with regard to the use of milk, as they

do in many of the cities on the other side, typhoid fever has been very nearly driven out. In Berlin and Munich, where there were severe epidemics some fifteen years ago, typhoid fever has disappeared now to such an extent, and is so rare, that the teachers in the medical schools and at the hospitals are scarcely able to find cases enough to teach their students the characteristics and appearances of the disease. I will mention here that the Germans rarely use raw milk; they will tell you that it is not fit to use, and I presume it is for that reason, and the efficient care that is taken of their water supplies, that cases are not plenty enough in those cities for the teaching of the clinical appearance and effects of typhoid fever. Within a month or two one of the most distinguished physicians of Baltimore stated before a medical meeting that if the water-supply and drainage of Baltimore was put where it should be and the milk that was used was sterilized, he would guarantee the disappearance of typhoid fever from that city within three years. Many of our prominent physicians believe that if we were to use no milk except that which is cooked or sterilized, typhoid fever would soon be as rare with us as small-pox.

Now, you see that this brings a very important public matter to our minds; if these things are true,—and I believe them to be true,—you can see the great responsibility that rests upon the boards of health and the people who have charge of our water-supplies, and also upon the boards of health who have charge of our milk-supplies, to see that the water and milk which is supplied to our citizens is free from contamination, if you stop to consider for a moment the possible effects of this contamination. Take, for instance, the water-supply of Boston: if there should occur a case or two of typhoid fever on the borders of one of the basins, and some of these germs should get into the water of that basin, the bacillus, being such a lively creature and having such vitality, would soon find his way through the water-pipes and the faucets to those who used the water. It is very fortunate those things have not occurred; and this is another reason why boards of health are so strenuous and determined in requiring that all cases of typhoid fever shall be reported to them. Fortunately, also, we have material, like corrosive sublimate and carbolic acid, which does destroy this bacillus. One of the most deadly mixtures which we can use for this purpose is the common lime-water that is used to whitewash fences. This is used at the hospitals, to be mixed with the excreta. Beside this, the most extreme care is taken that the persons who handle any of these dejections, or any of the

clothing about the patient, shall wash his hands so that he may not convey the disease to others.

Boards of health of the cities and towns carry on various investigations and inspections still further. The State Board began quite a number of years ago to employ Professor Sedgwick in the investigation of these epidemics of typhoid fever. He goes to work in a very methodical manner: visiting the houses where there are cases of typhoid, he sits down and asks all about each case; finds out where they get their milk; asks if this patient drank the milk, and whether other members of the family use the same milk; and with these facts which he obtains he is usually able to locate the origin of the epidemic. It is a very delicate and painstaking task to work the problem out, but you would be surprised to see how satisfactory the results are. Oftentimes you would say, "That case is not a milk case," and you would look for another cause. In my investigation of one of the Somerville cases this summer, when I asked the question, "Did you get the milk of such a milkman?" they said "No, but of Mr. ——." As there was no epidemic on his route, I said to myself, "Now, this must be one of those cases which must have been contracted in a way other than by the use of contaminated milk, and it will be extremely difficult to locate its origin;" but the question was settled when the woman remembered and said, "But that man does buy milk from the other man when he is short of milk himself." Another difficulty you have to contend with is the assertion by the milkman that no one connected with the handling of the milk has had typhoid fever. One of the Somerville epidemics illustrates this point: Mr. —— did not believe that his son who handled the milk was suffering from typhoid fever; but when the young man died, we were able to show by the autopsy that he had had typhoid fever. Another instance of the difficulty of getting the true facts is well illustrated by a case that occurred at an institution where they raise their own milk. They had three or four cases, the origin of which puzzled the doctors very much. They produced their own milk, and as they had no cases at their institution, they did not see how it was possible for any one there to contract the disease. It turned out, afterwards, that when they were short of milk, they occasionally bought milk from the milkman whose son had died from the fever, and that this milk was used only by those sitting at one table, and that all of the cases came from among those who used this milk, and that none of those who used the milk they produced had the fever. You see how much can be done in working out the origin of these

cases if sufficient time is taken, and you also see the reasons for the belief that many epidemics of typhoid fever are caused by contaminated milk. It seems to me that, to all intents and purposes, the proof is conclusive; at any rate, it is so accepted by medical men.

As I said in the early part of my remarks regarding water, that it might be filthy, vile, contaminated with all sorts of things, yet it would not produce the fever in those who drank it unless it contained the germs of typhoid fever. And so it is with milk. Milk may produce diarrhœa, may produce vomiting, might be contaminated with various bad things, but the use of it does not produce typhoid fever unless the germ of typhoid fever is present in the milk. This discovery we consider one of the triumphs of medicine,—this knowledge that typhoid fever is dependent upon this germ alone and nothing else. How and to what extent it is a preventable disease, you see, it is not easy to answer. I believe, and I think that almost all the medical men believe, that in all probability fully nine-tenths of all the cases of typhoid fever are caused either by water or milk that is contaminated with typhoid germs. If we should adopt the custom that the Germans have, and use no milk unless it has been cooked, I believe we would get rid of a very large proportion of the cases we have each year. It is not necessary that the milk be brought to the boiling-point to make it perfectly safe; it may be Pasteurized,—that is, brought to a heat of 60° or 70° C., and kept at that point for some fifteen or twenty minutes. If treated in this way, it is rather more palatable, as well as digestible, than if it has been brought to the boiling-point.

At the present time boards of health are doing all they can, and doctors are doing all they can, to find when there are epidemics of this fever; and now that the people understand this matter better, when there is an epidemic, they will either stop using milk altogether or have it boiled; and, besides this, if there is a suspicion regarding the water, they will boil that also.

With these facts that I have given you, I will leave you to answer for yourselves how far typhoid fever is a preventable disease. And I will also ask you why we cannot achieve as much in this line of preventive medicine as has been achieved in Munich and Berlin within the last fifteen years in lessening the large number of cases of typhoid fever that every autumn fill our hospitals and bring so much distress and suffering to patients, as well as to their friends who have to care for them and minister unto them?

DISCUSSION.

Dr. Smith.—Dr. Hildreth has very charmingly taken us from the mouth and the teeth to the intestinal tract. I would like to ask Dr. Hildreth if there is any good reason for a competent physician mistaking the diagnosis in typhoid fever? I know of a couple of cases where such a mistake was made. The patients were travelling abroad and were taken ill. They were supposed to have thoroughly competent physicians, and were treated some time before typhoid fever was ever mistrusted. It was not positively known until another physician was called and diagnosed the case as typhoid fever. From the facts which Dr. Hildreth has so ably put to us to-night we learn that typhoid fever is transmitted only through the excreta of patients who have that disease, and while he stated that the bacillus which produces typhoid fever bears a close resemblance to a harmless bacillus which is common in the intestinal tract, I believe he stated that it can be distinguished. Now, if that be true, why is it not possible, with proper care, to diagnose every case of typhoid fever as typhoid fever at once, without making an error which may lead to the loss of one or more lives? The doctor also remarked, in speaking of that case, of the son of the milkman, along whose route the epidemic had prevailed, that it was somewhat of a question whether the young man had typhoid fever, but the suspicion was confirmed at the autopsy. Now, my point is, whether the presence of the disease could not have been detected before the young man's death?

Another question occurred to me during the course of his remarks, and that is, What becomes of the bacillus on the patient's return to health? We understand that after the bacillus has been introduced into the system, it finds a favorable habitation in the intestines, where it begins to produce its toxin; the system immediately rebels and sets about to manufacture an antitoxin,—in other words, there is a war going on between the system and its antitoxin and the bacillus and its toxin. I suppose that war goes on until the bacillus is entirely stamped out and expelled from the system.

Still another question I would like to ask, How many of us are using cooked milk? I drink a great deal of water, and I feel quite secure in that respect, because I use "Poland Spring" water; but as to milk, I use it on my oatmeal or cracked wheat in the morning, and now and then take a glass of milk at my meals, but as yet have paid no attention to its cooking or sterilization. Of course, I have

been aware of its danger, but not so intensely aware as I am at this moment, and I would like to ask Dr. Hildreth and others if they use that precaution in their own households?

Dr. Hildreth.—The last question which the gentleman has asked is the easiest, so I shall answer that first by saying that I am one of those extravagant persons who have their own cows, so I do not have to give myself much concern about the milk that we use; but there have been times when I have not manufactured my own milk; and when such occasions arise, I sometimes have bought Pasteurized milk, and I have sometimes Pasteurized it myself, sometimes have it boiled, but never use it as we buy it from the dairy or grocery. The danger in some cases is very small and in other cases very large; we do not know where the danger lies, and I think there is some risk all the time. If you want to be on the safe side, the best way is to get into the habit of cooking your milk the same way as other articles of food. Sometimes, when a man is travelling, he will say to himself as he comes to one of those "ten minutes for refreshments" stations, "I will get off and get a glass of milk and a sandwich." Now, I would not drink a glass of milk in a railroad station for five dollars, and yet I am past the age at which there is an ordinary likelihood of my contracting typhoid. When I am in the country I never care to drink a glass of milk unless I know about it. My attention was first strongly directed to this by a remark of Professor Sedgwick, who said that he could not be induced to drink a glass of milk away from home unless it was guaranteed by some physician, or he knew it had been properly sterilized. Pardon me if I say a word right here about cooked milk. It is not quite as digestible as the uncooked. Milk that is fresh, within a few hours after having been drawn, is very digestible; milk that has been pasteurized or boiled is not quite so digestible, and that is something which has to be borne in mind.

The other question which the gentleman asked, that of diagnosing typhoid fever, was something that I should like to have alluded to, if I had not feared that I should have wearied your patience; but with your permission I will now refer briefly to it.

The question of making an early diagnosis is one of those very interesting problems where all the information we can obtain is not always sufficient to enable us to make a decision at once. Our first knowledge of the case is that the patient has been suffering for five or six days from malaise, headache, diarrhœa, weariness, some little fever and restlessness at night, and these prodromata may continue for a period of fourteen days, and then generally comes a

chill, and that is the time at which you generally date the beginning of the disease. Now, when the chill comes on, if it be a case of typhoid, you will find the temperature higher in the evening and a little lower in the morning in a perfectly regular way, and you will have a slight distention of the bowels, with frequent discharges which are yellow, like the dejections from infants. And yet, we may have all these symptoms, and not be able to say that it is a case of typhoid. In cases where there is such doubt, I have treated them just as though they were typhoid. I have observed all the precautions with regard to the destruction of the excreta, and have warned the families in such cases that great care should be taken to prevent the possibility of its spreading. Now, the question is, Why cannot the physician in the early part of this disease, during the first week, when the diarrhœa is pretty well pronounced, when it contains more of the bacilli than in the later stages,—why cannot he take the microscope and find out if the typhoid bacillus is there? If he found them he would say right off, You have got typhoid fever. Unfortunately, the making of the examinations of the stools, of the excreta, is somewhat difficult, and in hospitals, where they have the best facilities for that kind of work, it is not often done; they prefer to keep the patient under proper care and wait a few days for developments. Now, when the fever gets on to about the eighth day, you usually get rose-spots upon the abdomen; then you may be secure in your diagnosis. But suppose you have a case where the rose-spots do not appear? Then you think of acute tuberculosis, galloping consumption. You very often have a cough in typhoid fever, and this, with the fever and emaciated appearance of the patient, might lead you to think, This may be a case of acute tuberculosis.

With reference to the Somerville case, I did not intend to say that the physician in charge had any doubt as to its being typhoid; it was the patient's father, who insisted that his son did not have typhoid, but an autopsy was held which proved the physician was right.

An important discovery in regard to the serum of the blood of typhoid patients has been made by Widall, which now aids us wonderfully in diagnosing the disease. If you take a patient who has the disease sufficiently pronounced, so that his system begins to manufacture an antitoxin, to oppose the toxins of the typhoid bacillus, and prick his ear and get a few drops of blood (Widall, in his first experiments, drew thirty or forty drops), and put it under a little glass and allow it to stand a while, it

will separate into the serum and the clot; then take some of this serum and introduce into it a pure culture of typhoid bacilli, which I have told you were moving actively in all directions. If you observe this culture under a glass of the twelfth immersion, when this serum comes into contact with the bacilli they at once show that they have met with something they do not like. In three or four minutes they all huddle together as if they were trying to get away from something, and in ten or fifteen minutes they all remain perfectly still, as if they were overcome. The effect of this experiment is to show us that the serum that we took from the ear of the patient who was supposed to have typhoid was really manufacturing antitoxin, and that the patient who was supposed to have typhoid did really have typhoid. I have applied this test to almost every case that I have had at the hospital this fall, and with the best of success. The question comes, How early can we use this test, how soon does the antitoxin of the system become powerful enough to effect the bacilli of a culture? I think there is a doctor in one of the New York hospitals who claims that this effect can be obtained as early as the fourth day. You see this test may be very valuable when there is a patient in the ward about which there is a doubt. He has advanced, say, to the eighth day, and he has not had any rose-spots, and you are in doubt whether he is developing miliary tuberculosis, typhoid, or some similar disease. You draw a few drops of his blood, allow the serum to separate from the clot, and put the serum into a pure culture of the typhoid bacilli, and the very active germs, which are roaming in all directions, in the course of five or eight minutes are all moving together, and a few minutes afterwards there is not a sign of life in the whole of them. Now, how much this test will be perfected and to our knowledge of how best to oppose the toxin made by this bacillus it is difficult to predict. As I have stated before, we are not altogether hopeful of evolving a system of serum therapy which may effect the typhoid bacillus in a manner similar to that in which antitoxin acts on the diphtheritic bacillus. The effect of the serum of the typhoid fever patient upon the bacillus in its pure culture shows that if we could artificially produce this antitoxin we would be in as favorable a position with regard to the treatment of typhoid as we are now in the treatment of diphtheria.

Dr. Brackett.—I wish to express my admiration for that to which we have listened this evening. I have been glad to note the practicality of all that has been put before us, and happily surprised that the speaker should tell us that immunity from the risk of taking

disease from milk is so readily provided. I had supposed that brief boiling was not sufficient to guarantee destruction of typhoid fever germs. The matter of conveying disease through milk was brought to my attention several years ago by a lady who had a young son a student in an English school, and she said it was the custom of that school to boil the milk before using. In our household we have long practised scalding all the milk, and for us it is made more palatable by the process. Our water-supply is rain-water, carefully gathered and stored, and we do not drink much of that without cooking.

I would like to ask whether it is supposed that in contaminated streams or water-supplies, with the water at ordinary temperatures at any season of the year, there goes on proliferation of typhoid germs, and what is known concerning their reproduction outside of the human body.

Dr. Hildreth.—It is not altogether clear to biologists whether they do propagate in large quantities or not. How and where they propagate outside of the body has never been discovered. You can grow them in some media, but on account of the great activity of the germ no one has ever seen the process of reproduction, or knows how or when it takes place.

Dr. Briggs.—I would like to ask one question in regard to the original contamination of the milk-supply,—whether Dr. Hildreth thinks it is due to the cow drinking contaminated water, or to the handling of cows' udders by men who have not taken care of their hands, or possibly to the water which may have been added to the milk, or to the water which may have been used in rinsing out the cans?

Dr. Hildreth.—The majority of cases are undoubtedly due to the hands of men who do the milking or who handle the milk. It is possible for a cow to transmit it simply from drinking contaminated water. The number of cases would probably be small that could be traced to the rinsing out of the cans or the addition of water to the milk.

Dr. Briggs.—Then, in other words, if the hands of the men who work about the farm, and who handle the milk, are clean, and there is no water added, you would expect no danger from that milk?

Dr. Hildreth.—Not from typhoid.

Dr. Williams —I would like to ask Dr. Hildreth if observations or investigations have been made the results of which would lead one who has had typhoid fever to hope that he has become immune from typhoid hereafter?

Dr. Hildreth.—The fact that typhoid fever is so rare the second

time shows that a person who has recovered from an attack of it may expect a reasonable degree of immunity. In that respect it is different from diphtheria. A person may recover from diphtheria and the immunity not remain more than two or three months, and in most cases it does not remain longer than a year or two. This has been proved by the examination of the serum of patients who have recovered from diphtheria; it has been observed that the antitoxin of the system disappears after a little while, and the patient is again susceptible to the disease. The immunity is much greater in typhoid, but unfortunately is not complete. I have known of a case where a patient succumbed to the third attack of typhoid fever.

Dr. Clapp.—I would like to ask Dr. Hildreth if the Pasteur filter or the Boston filter excludes the bacilli of typhoid? I have a personal interest in asking this question. For the last six years I have used in my family water of the Boston supply, but it has all been filtered through a Pasteur filter or a Boston filter, and I have used the latter for a large portion of the time. It is attached to the pressure and connected with a glass globe that holds about two gallons. It is arranged automatically, so that when a certain amount of water enters the globe the supply is cut off. Now, we sometimes use that globe for perhaps three months without cleaning it out, and at the end of that time there is not enough sediment in the globe to soil a white handkerchief if used to wipe it out.

The reason that I have used water in this way in preference to the celebrated springs of Boston and vicinity is that to my mind I am surer of obtaining pure water, and I can tell you a little story which I think you will agree fully justifies my belief. A gentleman that I know was very much enamoured of a certain well-known spring water; no other water would do, and he used so much of it that he had it come by the barrel, and after using a portion of one of the barrels he thought he noticed something wrong about it, and an investigation showed a mouse in the barrel. A mouse will not go through my filter.

Dr. Hildreth.—With reference to the Boston filter, I cannot speak very certainly. It seems to me that it should be a good filter. The other day my attention was called to it, and this same question was asked me, and, while I have never used one, I have examined it carefully; had the cover taken off and looked all through it, and it seems to me it should be a good filter, and that the water that passed through it should be sterile.

With reference to the Pasteur filter, when they were first intro-

duced, scientific men were very much interested to see if the water, after passing through it, was really sterile water. Many experiments were made abroad, and at the Institute of Technology, with their large and completely equipped laboratories, they made a series of investigations and found that the number of bacteria in the filtered water was very small; the most they found in any of those experiments was two-tenths of one per cent., which is practically pure water. I have used one of the Pasteur filters for some time in my house. The president of the State Board of Health, Dr. Wolcott, has given his attention very largely to this matter of pure drinking water, and he is a better authority on the subject than any one that I know, and it is reported that he says he would be perfectly willing to drink the water of Chicago, which Professor Sedgwick had examined and found to contain several different kinds of dangerous germs, if it only went through a Pasteur filter. I think a Pasteur filter is absolutely germ-proof.

Dr. Barker.—Speculation is sometimes interesting if it is not profitable. We have heard several things this evening which might make good subjects for speculation, and one of them is, Where is the native heath of this species of life which we have heard so much about? We know that the home of the elephant is in Asia, the home of the lion in Africa, of the kangaroo in Australia, etc., but where is the home of the typhoid bacillus? Where do they come from? How do they get into the water or the milk which we drink? Do they grow there spontaneously? Scientists tell us that such a thing is not possible. The same power that made a bacillus made a banyan; the same power that made a micrococcus made the moon. You know it is said that "great fleas have little fleas upon their backs to bite 'em, and these again have lesser ones, and so on, *ad infinitum*." We must get water into the system and we must take food. The doctor has told us that if we wished to be sure of escaping germs we must not drink milk or surface water unless it has been boiled. Now, it is impossible for most places to get water from subterranean sources by vast systems of wells, driven so far below the earth's surface as to be below contamination. That is one thing that I think our municipalities and boards of health are remiss in, not directing attention to the advantages of subterranean sources of water. How manifestly impossible it is to get pure water from the vast water-sheds that feed our water-supplies! We find that cattle, horses, and sheep graze and live upon the hill-sides, and when there comes a rain everything on these hill-sides is washed into the rivers and brooks which so many

people are obliged to use for drinking water. It is a comfort to know that if we are obliged to drink surface water that we may escape danger by boiling the water, but above all and beyond all, and it is a point I think is too often lost sight of, we should aim to keep ourselves in such a condition of health that these germs will have no effect on us. It seems to be conceded that the man who is a vital man, who is brim-full of light and health, enjoys immunity from their attacks. That is one way, and that, in my opinion, is the better way for us to combat them. Lice, as we know, attack weak, puny plants, they attack animals that are weak and sick. The parasites may be there at all times, but they do not attack the plants or animals successfully until the vitality is low. Wherever you find animal life you will find it has its parasites, and the most successful way to combat them is, if possible, to get within so much life, so much vigor, that they will be unable to get a foothold.

In my own household I do not feel it necessary to use the precautions which the doctor mentions, as my water-supply comes from a point sixty-nine feet below the surface and through thirty-five feet of dense hard pan, amounting almost to slate. With regard to the milk-supply, like our friend, I keep a cow in my stable, and I try to keep her clean enough so that she might be brought into the sitting-room if it were ever necessary.

Dr. G. T. Baker.—If I understood Dr. Hildreth, he stated that after forty years of age a man was practically immune from attacks of typhoid fever; how is it with extreme youth, or babies, especially those that are bottle-fed altogether?

Dr. Hildreth.—It is not known exactly at what age they become susceptible to the disease, but I think it is very probable that even very young babies, say two or three months old, are sometimes attacked by it. The cases are not very common until about five years old,—that is to say, the hospital records do not show anything much under that age, but I sometimes think that many cases are attended at home and may not be known as typhoid. I saw a case this summer of a child not more than six months old who had been sick with a disease which ran a course much the same as typhoid. The cases are comparatively rare under four and after forty, although I have known of a case of a man dying at sixty-five, and the autopsy showed the cause to have been typhoid fever. The most susceptible age is from twelve to twenty-five.

Dr. Briggs.—If the danger to milk is from handling, why is not there the same danger in handling of waters, as in bottling or bar-

relling? why is not there just as much danger of it by a man suffering from walking typhoid?

Dr. Wilson.—I would make a motion that a vote of thanks be extended to Dr. Hildreth for the very interesting and instructive talk he has given us this evening.

Unanimous vote.

THE regular monthly meeting of the American Academy of Dental Science was held at Young's Hotel, Boston, Wednesday evening, February 3, President Andrews in the chair. Subjects for discussion:

I. Retaining Appliances in Orthodontia.

II. Dental Materia Medica.

DISCUSSION.

Dr. Briggs.—Mr. President and gentlemen, that which occurs to me more and more in practice—and it is quite opposite to views that I once held—is that the dentist must more and more fit himself to practise medicine, in the line of prescribing, to a certain extent, drugs for his patients. In the old days, before the hypodermic syringe, and when certain drugs were the only ones to be found in the dentist's armamentarium,—when morphine was the only analgesic, and that was too dangerous to use unless one could watch its operations,—it did not seem proper for a dentist to prescribe the use of drugs where the circumstances would be such that the dentist could not see the actual result, and thus protect the patient from any harm which might result. Times have changed; there has been an influx of the coal-tar products, and of combinations of cocaine, which, whether they are dangerous or not, the laity have no hesitation in using, sometimes in quantities and under conditions where a dental practitioner would not.

The thing that is forcing us into it, perhaps more than anything else, is the fact that the physician refuses to be educated in dental matters. Time goes on, and no attention is paid in the medical schools to teaching anything about the mouth or the teeth, while the dentist himself is getting broader, and is reaching out more into the domain of medicine. This tendency of the physician to ignore the dentist and his work is exemplified to us every day, and he probably knows as little to-day about the effects of these coal-tar products on the pains and inflammation with which we come in contact as the majority of the patients themselves.

Apropos of the feeling of the physician that the domain of the dentist is not in his line, I will mention a case of a physician who was called to a lady who was confined to her bed with an abscessed tooth, the abscess having formed in the roof of her mouth. The physician observed it, and said, "You must send for your dentist right away." When asked if he could not attend to it and relieve her suffering, he said no, it was something for the dentist to

do. A messenger was sent to me, who explained the case, and I sent an associate, who lanced the abscess, and gave directions for the treatment until the lady could visit the dentist. The ridiculousness of a physician stating that the treatment of that abscess was different from an abscess anywhere else shows their limitations in matters connected with the mouth. Of course, we know that the principles of treating an abscessed tooth, as far as the immediate emergency treatment is concerned, are just the same as those which underlie the treatment of an abscess in any other part of the body. And so it is when a person is suffering severely from pulpitis, pericementitis, or alveolar abscess, and the physician is sent for; he is perfectly helpless, and does not know how to go to work to relieve the patient, and unless we step in and give the proper treatment the patient must suffer in consequence.

The hypodermic syringe has brought obligations upon us. If we give anæsthethics, we must be able to use the hypodermic syringe to revive patients from shock or from being overcome by chloroform or ether; then we very easily use it in many other conditions to relieve pain and assist operative interference. Our knowledge of the use of the hypodermic syringe in many of the minor surgical operations about the mouth has brought about an enlargement of our field, and has led us to do what we have hitherto not attempted, because it involved the general anæsthesia of the patient.

So, to sum up, it seems to me that instead of teaching, as I have always done, that we wanted the dentist to know materia medica in a broad and general way, as a matter of education, and to point out at times the necessities of the patient to the family physician, it has come to the point that, if the dentist is to take full charge of his department, he must be capable and fitted to apply the entire materia medica,—not "dental materia medica;" there is no dental materia medica; materia medica is the same for the dentist as it is for the physician, and the same principles that underlie the treatment in any part of the body apply to all the conditions with which the dentist meets. I hope what I have said, Mr. President, will start a discussion.

Dr. Eames.—I have brought for your inspection a few of the advertising leaves from the January number of *Items of Interest.* On one of these "Dr. Hayes's Improved Process of Anæsthesia" is advertised, and I know that very many dentists are deceived, and influenced to pay one hundred dollars for an "office right" use of preparations, of the composition of which they are utterly igno-

rant, and to administer them to patients for the production of anæsthesia. It is a serious thing to administer any anæsthetic: the patient is rendered unconscious; the powers of life are, one by one, suspended, approaching the very doors of death, yet this secret mixture is held up to the profession as entirely harmless.

I think that we ought to put the stamp of condemnation on these and other secret remedies, such as "Uzane," "Paralesia," "Noxall," and others, and have it understood that this society disapproves of advertising them in professional journals. Although our society has a record of action against secret remedies, it seems necessary to act again, and I hope something will be done to-night.

I would now like to refer to an old remedy that has been revived by myself,—that is, rapid breathing as a pain obtunder. We all know that Dr. Bonwill was the first to introduce it. I happened to be present when he made a demonstration in the Pennsylvania Hospital in 1876, but I have never used it until within the last three years. One of my first operations was the removal of a sebaceous cyst. I made a free incision across it, turned out the sac, and dressed it, without the slightest pain to the patient. I have extracted teeth in sufficient numbers to perfectly demonstrate its efficacy, and have had no ill results. I do not use this method indiscriminately, but select the patient.

Now, a word in regard to the coal-tar series of remedies. We receive numerous samples and circulars regarding these preparations, but we do not always know their composition. I think it is much better, in such cases, to prepare our own remedies. For instance, ammonol is a good sedative theoretically, but practically it is unreliable. I would prefer to prescribe acetanilide with the proper amount of ammonium carbonate, freshly prepared.

We have a new product, eucaine, which is advertised as a substitute for cocaine. I believe we should test this more fully and for a longer time before accepting it as perfectly harmless. I have some reasons to doubt that it is perfectly harmless, at the same time I have used it in quite a number of instances during the past eight months. In one case I used it freely to extract five roots of teeth at one sitting, injecting it very freely, without any after-effects. I do not think that, applied to mucous membranes, it is quite as effective as cocaine.

I have been making some experiments upon animals, which I have not yet completed. As far as I have gone, however, eucaine seems to be more toxic than cocaine, killing a frog which had recovered from a similar dose of cocaine.

Dr. Banfield.—Will Dr. Eames tell us what per cent. and how much he injects of the eucaine?

Dr. Eames.—Usually a four-per-cent. solution.

Dr. Piper.—I would like to ask Dr. Eames if he has had an opportunity to notice the after-effects,—the swelling which he said occurred the next day?

Dr. Eames.—You will remember I mentioned in passing that it is claimed that there is an infiltration of tissue, and it is admitted that a swelling may occur the next day, which is painless and harmless. In my own cases I have not had marked swellings.

I do not feel that I can agree with a recent statement in one of our journals in regard to the Schleich method of infiltration, which is an excellent method of producing local anæsthesia. The statement was made that with a solution of one grain of cocaine to the ounce you might inject an ounce without any fear of the result. That would mean a grain of cocaine, and I do not think that a grain of cocaine should be injected into the system with impunity by the Schleich or any other method.

Dr. Adams.—I would like to ask Dr. Eames if he has ever tried the rapid breathing in the excavating of teeth.

Dr. Eames.—Not enough to speak definitely about it.

Dr. Wilson.—I would be pleased if Dr. Eames would speak a little more in detail in regard to the method of rapid breathing. I have heard it spoken of a great many times and never paid much attention to it, but if there is really anything in it, I should like to know just what can be done. To be entirely candid, I never supposed it amounted to anything.

Dr. Eames.—I had in mind to bring a patient here and do a little operation, but I did not think that this would perhaps come into much prominence, and hesitated about taking the time of the Academy. I feel that in regard to sensitive dentine it might not be used so much, because it is hard work to get many patients to co-operate. You have to use a good deal of energy yourself to get the patient to make sufficient effort to bring about the result; you have to constantly urge them to breath rapidly. Its very simplicity makes the patient unwilling to attempt it. You have to be convinced of its efficacy in order to influence them. Until I was convinced, I did not urge it, but now, unless the patient very decidedly objects, I insist upon it. It lasts a very short time, and if the operation is to be extended, you have to ask the patient to continue breathing.

Dr. Payne.—Dr. Eames spoke of selecting his patients. I would

like to ask him if there are any dangers connected with it that it would be well to be prepared for?

Dr. Eames.—In case of a patient with weak lungs there might be a liability to strain, which might be productive of some injury.

Dr. Stevens.—I wish to substantiate all Dr. Eames has said in regard to rapid breathing. I know that it is wholly true, and the way it came to my attention first was perhaps twenty years ago; one time when the gas was all out of the tank and a patient came in to have a tooth extracted and wanted to take gas. After the first two or three inhalations the gas gave out. I knew it would be useless to attempt to explain matters, so I allowed the patient to continue breathing air instead of gas, and it had the same effect. I was talking to Dr. Bonwill one day, and told him about this, and he corroborated my statement very heartily. He told me how he had made the discovery himself in a very similar way, and while out walking one day he began to think over the matter, and got so interested that he sat down on the curb-stone and made a study of it right there in the street. He got very enthusiastic over it, as many of you may remember. The only trouble about it is to make the patients believe it. It is such a simple thing that they will not believe when you tell them that it is only necessary for them to breathe rapidly for a few minutes and you will be able to extract their tooth for them without pain.

Dr. Clapp.—I have been very indignant for a long time at the character of the advertisements which appear in the dental journals. I am glad that Dr. Eames has brought this matter up, and, just to test your opinion with regard to it, I will introduce resolutions, which you can think over and discuss, and if you wish, I will present them in the form of a motion.

"*Resolved*, That the officers of this Academy be instructed to draw up a resolution condemning the publication in all professional journals of unprofessional advertisements.

"*Resolved*, That this resolution be forwarded to the leading dental journals as expressing the sentiment of this Academy."

Dr. Eames.—I should like very much to see a motion of this sort carried. In talking with the head of the Consolidated Company here, he asked me how I liked the journal. I replied that its articles, contributions, etc., were very satisfactory, but to have them associated with the cuts and advertisements which appear in the journal I considered incongruous and unprofessional. He replied

that the journal could not live without its advertising pages; and I have thought since that if we cannot support a dental journal without its being associated with these quack advertisements, we had better do without it altogether.

Dr. Banfield.—Who is to be the judge of the advertisements, as to whether they should be allowed to appear in a dental journal? What we might consider questionable advertisements they might say were all right.

Dr. Clapp.—It certainly might be difficult to draw the line, but it seems to me that an expression of this kind would be pointed enough for the journals to understand, somewhat, our sentiments. Now, Dr. Eames has made a remark which has impressed me very much,—that is to say, he has quoted a remark made by one of the officers of the Consolidated Company, that the "journals could not live without the advertisements." It is a very great question in my mind whether it is necessary to have so many journals live; the sooner some of them die the better. We do not need many journals to take care of all the choice dental literature that is presented for publication, and I do not see why we should feel any obligation or desire to support two or three in Boston, five or six in New York, seven or eight in Philadelphia, fifteen or sixteen in Chicago and the West. For my part, I do not see why they should be supported.

Dr. Smith.—This matter can be looked at from two stand-points: we look at it from a professional point of view, while the Consolidated Company view the matter from a business stand-point. They are serving, as they think, their customers. It is strictly a business transaction, and to the person in business all advertising is legitimate which can be safely carried through the United States mails.

The remedy for this evil is in our own hands. The support of these journals rests largely with the profession, because it is through the profession that the advertisers and publishers expect return for their outlay, and if there is anything we dislike about the journals, all we have to do is simply to refuse to take them, and refuse to publish anything in them, and the matter would soon adjust itself. If the profession would take concerted action in such matters as these, they could correct many things which they dislike. How difficult this is to accomplish, even among those we term the best and highest of the members of the profession! I have no doubt that the journal in question is a good one, but it is published in the interest of a dental manufacturing company, whose object is to bring the goods they manufacture before the dental profession. Now, we

have a dental journal that is published by the profession and for the profession. I shall not attempt to say what success we are having, but I will say that so far the road to success has been uphill, and why? Because many of the men who take the INTERNATIONAL DENTAL JOURNAL, and who will tell you that they believe in its objects, when they have a paper which they consider has some merit to it, go and publish it in the *Dental Cosmos*, because they want it to reach the greatest number of readers, and an article published in the *Dental Cosmos* is read by almost the entire profession. If these men would publish their papers and ideas in a strictly professional journal, the dentists, who are ready to support anything, would soon find out that it was for their interest to take these professional journals published by professional men, and the withdrawal of this literature from the trade journals would deprive them of their value, so that the profession would cease to support them.

Dr. Clapp.—I believe that, and I believe we should do more. I believe we should let these journals know our sentiments. And for that reason I have thought the method proposed would be an effective way of bringing it to their attention. It has been suggested that the advertising in the dental journals was a business matter in the same sense as that of the various trade journals. I do not think that is so. The advertisements in the various secular papers of ordinary business do not degrade anybody, but the advertisement in professional journals of unprofessional remedies and methods does degrade the dental profession, and it is one of the most pernicious examples that is presented to our young men as they enter into the practice of dentistry. I think our disapprobation of such methods should be made known as the sentiment of the American dentist, not the Boston, or the Massachusetts, or the New England, but the American dentist.

Mr. President, I move that the resolutions which I have read be adopted by the Academy.

Dr Smith.—I heartily second the motion. Certainly there can be no harm, but great good, in expressing a sentiment of this kind.

President Andrews.—I put this motion with a great deal of pleasure. It is rumored that the privilege of the publication of the transactions of one of our largest and most active dental societies has been sold to a trade journal for several hundred dollars, —I do not remember the exact amount. It seems incredible that a society of professional men should withdraw their support from a professional journal and transfer their proceedings to a trade

journal for a money consideration, and it is with much pleasure that I put this motion, to adopt the resolution which Dr. Clapp has offered.

Dr. Wilson.—I believe the motion is a good one, and while it may appear to some that the matter of advertisements is something which should not concern us, yet it is in line with keeping up the standard of the profession. What our President has said about a dental society selling their transactions was news to me.

It seems to me that if we are earnest about this motion that has been made, we might consult with other societies and see what they will do. We ought to push the thing a little further than simply acting upon it ourselves. I heartily believe in it.

President Andrews.—The matter to which I referred has made such a stir that many of its most influential members have left the society. Are you ready for this motion?

Unanimous vote.

President Andrews.—We have two subjects for discussion this evening, on the first of which, "Retaining Appliances in Orthodontia," Dr. Smith will open the debate, but before speaking on that subject he has another matter which he would like to call to the attention of the Academy.

Dr. Smith.—Mr. President and gentlemen of the Academy, preparatory to speaking on stay-plates used in orthodontia I would like to present a case showing the value of the shadowgraph in our work. The case is represented in these two models. This one, marked B, shows the condition of the mouth at the time of the eruption of the upper right lateral incisor. The left central and left lateral came through in their proper places, and when the patient was about eight years of age the case came into my hands. All I could then know was that a tooth was presenting itself, there being a fulness of gum-tissue, and I understood that the lateral incisor had previously loosened, and that the patient's mother, or some one at home, had taken it out. The temporary central incisor was also very loose, and was extracted. The permanent lateral then erupted, and took a position inside the line pointing towards the central. Appliances were made to bring the lateral tooth outside of the lower incisors, and we waited for developments. This model shows the case in 1891, and we have watched with interest this case ever since, hoping all the time that the central incisor would soon come down. The other day the patient came in, being quite elated at seeing the point of a tooth directly over the lateral, thinking it was the central. A few days later the tooth had come

through sufficiently for me to see that it was the permanent cuspid. The temporary cuspid was extracted, that being somewhat loose, and I was then very much in doubt as to what to do, having waited so long for the central to come down. I have not had time to have a cast taken showing the mouth just as it is at this moment, but this mark shows very well the cuspid tooth now picking through directly over the lateral. I got the consent of the patient to go with me to Dr. Clapp's and have a shadowgraph taken, and this work of Dr. Clapp's shows up as rather the best he has done, and we all know that he has accomplished some very good work in this line. From it you will see that there is a tooth here, the necessary right central incisor between the right lateral and the left central, that is turned half round. With this explanation, which it seems to me all that is necessary, I will pass these models about with this shadowgraph.

Now, as regards retaining appliances.

The remark that is frequently made that the correction of an irregularity is easier than the permanent maintenance of the results, I do not think is true. The disappointment which has resulted in many well-planned cases of orthodontia has been due to the too early application of retaining appliances. I think that is an error we all make. I made it myself, and by experience I have learned that after accomplishing a change in the teeth, if you allowed the appliance that did the work to remain for some time after the teeth have been brought to a satisfactory position, so that the new bone may be deposited, then the newly made appliances for retaining more readily holds the teeth in their changed position.

There is no question but what the best retaining appliance is a fixed appliance. To illustrate this, you take, for instance, a case of protrusion of the anterior teeth which have been brought into line. If an appliance can be made by putting bands on the molars and cementing them to place, with a wire running from the molars around the front of the teeth, the ends being soldered to these bands, you have the ideal retaining fixture; but it is rarely that you can use these fixed appliances,—not because it is a mechanical impossibility by any means, but because patients object to retaining appliances that cannot be removed for certain important society events. Many of the patients for whom we are called upon to do regulating are young girls just going into society, and whenever anything of the kind has to be made, the families of the patients insist that it shall be something that can be removed when it is

thought desirable to do so, and so I find that to accommodate this great demand of society I am obliged to devise some appliance which the young lady can remove just before stepping into the ball-room, and put on again immediately after returning home. Of course it takes longer for the teeth to become fixed in this way, because the tendency of the teeth to go back to their old position will assert itself upon the removal of the pressure, even if only for a few hours, until such time as the new tissue can hold them firmly in the desired position, and yet that is all we can do under the circumstances. But, as I said before, the fixed appliance is without question the ideal appliance, and the removable retaining appliances are, as a rule, better made with some form of vulcanite with metallic attachments. If a crib appliance is used, which many seem to prefer, they have to be made quite heavy, for if they are not, you will find that patients in taking them out and putting them in are apt to get them out of place, and when they do not succeed in getting them into place without much trouble, they will be laid aside.

Of course, it should be the aim in making a retaining fixture to have the smallest possible contact with the teeth in order to prevent the possibility of decay. The retaining appliance shown here by Dr. Baker a few years ago was an ideal one of its kind. It requires great skill to make such an appliance and have the bearings accurate, and in cases where teeth have only been moved out or in, an arrangement of that kind is admirable. The idea is a modification of one that is presented by Dr. Farrar in his work on Irregularities, and for the very skilful adaptation of the idea to his individual case Dr. Baker certainly deserves a great deal of credit.

In regulating cases, where we have simply moved an instanding incisor out over the lower incisors, many times the occlusion itself is sufficient to retain the tooth in position, but sometimes you will find that after the correction of the irregularity your incisor still drops back a little to an occlusion with the lower. Now, such a case as that could be nicely retained by a simple fixture, say a band around the tooth with wire projections resting on the adjoining teeth, and yet you will find it is the desire of most patients to avoid wearing such an appliance, and your only recourse, therefore, is to again resort to an application of the vulcanite plate, with little wire projections behind the tooth keeping it from moving back.

The Watt plugs, which writers upon this subject allude to, is a method that I do not think is very much employed. I confess I

never saw a case except in the pictures. When you look at the pictures and read the description of them, it would appear to be a practical thing, but to my mind it is not so feasible as it would seem. If you are intending to hold a tooth in place with a Watt plug, it is necessary that there should be a cavity. If there is not one there already, you must drill into the sound tooth and make one, and I very much doubt the expediency of mutilating a tooth for the sake of making a place to hold this plug, and I think, notwithstanding all that has been said to the contrary, that a vulcanite plate is far superior to the Watt plug method of holding teeth in place.

There is another point in regard to this matter of retaining appliances of which we often lose sight, and that is the tendency of regulated teeth to return to their old form of irregularity. There seems to be a natural tendency in that direction which in some cases it seems to be almost impossible to overcome. The teeth are moved into place; retaining appliances are put on and worn by the patient for a year or more, and then taken off, and back the teeth go to their old position, and the entire work is regarded as a failure, when really the only error was in the fact that the retaining appliance was removed too soon. In other words, there are many cases of corrected irregularities where the apparatus for retaining the teeth in their changed position should have been a retaining appliance for life,—a permanently fixed appliance, which should be allowed to remain in the patient's mouth. That is what we lose sight of. If I understand the object of our work in prosthetic dentistry, it is in part to make people better looking. If it is professional to extract natural teeth for the purpose of supplying artificial teeth and improving a person's appearance, it is certainly quite as professional to make a fixed retaining appliance by cutting into the teeth and anchoring them by wires to hold them in the desired position, with the same end in view. There are cases where we are forced to do this. Take, for instance, a case where we have a protrusion of the incisor teeth in a patient somewhat advanced in years, say about thirty, and you have drawn them back into line and wish to retain them. The usual method of retaining in such a case is to drill into the palatal surface of the teeth and set wire retainers which rest on the adjoining teeth, so that it is impossible for those teeth to move out. Now, the tendency of those teeth to move out, especially in a person of that age, is something astonishing. I recollect a case of the kind where two central incisors had been drawn back and wire re-

tainers, resting on the laterals, were put in, supposing the laterals would be sufficient to hold them. They not only moved forward themselves, but they took the laterals along with them. A new appliance had to be made for correcting, and the same kind of a fixture was made for retaining, except that this time it was extended to include the cuspid tooth, and there was no further trouble.

The many applications and modifications of these crib arrangements are to be governed by the individual case and the ingenuity of the operator. With so many different appliances pictured in the books, I think one shows quite as much skill in the proper selection to suit the case in hand and in carefully adapting it as he would if he set about to originate something new himself. In many instances, after spending a great deal of time and thought on some particular operation, he will find that his ingenious production is simply an old idea worked over, with some modification of his own, adapting it to the individual case.

The disadvantage, of course, of all vulcanite work for retaining is that there is so much surface in contact with the teeth that you have a great liability to decay, at least so it is claimed, and yet, out of a great many such appliances that I have put on, there have been very few, comparatively, with which I have had any trouble on account of decay; in fact, at this moment I can recall but one, and that was a case where a dentist extracted the sixth-year molars because they were badly decayed; but as the case turned out, it would have been better to have kept the molars in and crowned them. The arch had been spread to correct an irregularity, but it would not remain, and it was necessary for the child to wear a vulcanite plate constantly, and in that case the teeth decayed; but it was the only one that I remember having seen in which decay resulted from a vulcanite plate for retaining.

I think I have briefly covered the general principles of retaining appliances, but after the discussion has started, it may bring something to my mind of which I will be glad to speak later.

Dr. Ainsworth.—I wish to say "amen" to the remarks of Dr. Smith. I agree with him in regard to leaving on the appliance with which the teeth have been moved for some time after they are in a satisfactory position before undertaking to make a permanent retaining fixture, but I find a common reluctance among patients to wearing it after the teeth are in position,—that is, the appliance with which they have been moved,—and I have been many times influenced by their wishes, much to my regret.

There are four things, to my mind, that are important in a retaining apparatus: it should be positive; it should be as cleanly as possible; it should be comfortable; and it should not present an unsightly appearance.

As Dr. Smith has said, we find among the class of people for whom we are called to practice this specialty a reluctance to wearing anything that is conspicuous, and we are constantly called upon to exercise our ingenuity in making something that shall be as little noticeable as possible; but such appliances have in the main proved unsatisfactory.

A fixed appliance, it seems to me, is far preferable to a removable one, because it is much more positive, and you are sure that it is being worn twenty-four hours a day, while a removable one may be in the pocket twelve hours a day, in which case it will take a long time for the teeth to become fixed, if, indeed, they do at all. They will be very likely to go back to their former position when the appliance is finally removed.

An appliance should be made so as to have as little contact as possible with the teeth, to prevent injury and facilitate cleanliness.

I hold in my hand a case for which I had made this gold retaining-plate, designed to be unconspicuous and to please the patient. The arch had been spread and the front teeth drawn in,—a case of "thumb-sucking." This plate was put in last spring, and I discarded it in the fall as practically a failure: it was not sufficiently positive; then, too, we needed all the room between the incisors to hold them down to place, and the little gold projections on the plate coming between the incisors kept them slightly out of line. This case was one in which the patient seemed unusually disturbed lest she should have to wear something which would show too much. I finally hit upon this compound, and solved the problem in the following way, which is proving eminently satisfactory; and one of the requisites was that she should be able at times to leave off the part which was noticeable:

I made gold bands for the two cuspid teeth, also for the two sixth-year molars; shaped a sixteen-gauge platinized gold wire to correspond with the arch and rest against the palatal surfaces of each tooth; this arch band was soldered to the bands encircling the cuspid teeth, and the ends engage in tubes soldered to the palatal surface of the bands encircling the molars; the molar bands were then cemented into position, after which the cuspid bands were cemented in place, carrying with them the arch band, the ends of which engage in the short tubes on the molar bands; this serves to

hold the bicuspids and molars from dropping in, and to hold the incisors in. I soldered short pieces of gold tubes in a perpendicular position to the distal buccal surfaces of the bands encircling the cuspids, then, forming a smaller platinized gold wire to fit the labial surfaces of the front teeth, I bent the ends at right angles so as to engage in the little tubes attached to the cuspid bands; this front band can be taken off and snapped back in place at will. When off, nothing shows but the bands on the cuspids. Of course, in cases of this kind the front teeth must be held down in position a long time. In time I anticipate the patient will get along all right by wearing the band at night only.

Dr. Cutter.—I have nothing special to say upon the subject, which has already been so fully discussed.

I would, however, state that I have rarely seen any injurious effects from vulcanite and wire retaining-plates coming in contact with the teeth, yet I am always particular to instruct patients upon the importance of keeping such appliances as clean as possible.

Dr. Smith.—I will simply state that the case where Dr. Ainsworth spoke of having made a satisfactory appliance by placing anchor bands upon the cuspids, with a wire passing along the palatal surfaces of the anterior teeth, was good as far as it goes, but it only goes to the cuspid, and that is fatal. In the great majority of cases you would find that people would object to the bands on cuspids; they do not want to have any gold showing. And then, again, the objection might be offered that the wire on the inside touches the palatal surface of the teeth; if fairly large, I should fear the chances of decay in these much more than from a vulcanite plate that was removable. I have seen a number of cases where two wires were used,—one passing along the inside of the teeth, being ligated to the cuspid, and the other passing on the outside, ligating the tooth which was to be carried into line; and I have seen more harm done in that way than could possibly have been done by a vulcanite plate pressing against the palatal surfaces of the teeth.

Dr. Ainsworth.—I would say, in answer to Dr. Smith, that the band is not a flat wire, as it appears in the cast here. It is a round wire, and does not touch the teeth except in spots that will be in the future entirely exposed to friction, where there is no ordinary tendency to decay and no fear of decay. The teeth are good and strong, and the position of the band is such that it is cleanly. If there were ligatures worn here, there might be trouble, but I have no expectation at all of this band doing any harm.

THE regular monthly meeting of the American Academy of Dental Science was held at Young's Hotel, Boston, March 3, 1897, at six o'clock.

The paper for the evening was read by Gardner T. Swarts, M.D. Subject, "Diphtheria."

DIPHTHERIA.

BY GARDNER T. SWARTS, M.D.[1]

I feel, of course, highly honored to speak to you on any subject whatever, but feel especially pleased to think that gentlemen in your line of work should devote some time to learning something about a disease in which I have been especially interested. Of course, men in the medical profession should be specially interested in all diseases, but the question of typhoid fever and this subject of diphtheria are topics that are of especial importance to us, because of the great number of human lives involved.

I am sorry on account of the regularity in the publication of your transactions that I have no paper. My apology for not presenting one is that the subject, being rather dry, the reading of a prepared paper might be recommended as a cure for insomnia, and that is not the effect which I wish to obtain this evening.

The word "diphtheria" comes from the Greek, and means "a tanned hide." The reason for that, I presume, is the resemblance of the characteristic form and texture of the membrane which is found in this disease, which we have called diphtheria. As we come later to the clinical symptoms, we will find that this analogy is very close.

Diphtheria, as the disease diphtheria, may have been with us for centuries, but that claim is disputed by many able men, especially the older practitioners, who practised back in the fifties, who give us the historical information that this disease, with this peculiar formation of membrane, did not make its appearance in the United States until about the year 1857; that previous to that time we had sore throats, tonsillitis, pharyngitis, laryngitis, quinsy,—we had various formations in the throat and inflammatory conditions there, but nothing like that which we now characterize as diphtheria and with which peculiar constitutional symptoms occur. This disease in the production of its symptoms is very character-

[1] Secretary of the Rhode Island State Board of Health.

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istic, and when a typical case occurs is not mistaken for any of the other conditions; but, unfortunately for the patient as well as for the physician, those constitutional symptoms, by which we can determine whether the disease is or is not diphtheria, very often do not present themselves until possibly we are about ready to sign the death certificate. In fact, it has, up to within two or three years, been a common saying among physicians that if a person presented himself to a physician with a tonsillitis or an ordinary sore throat and he wanted to make sure of his diagnosis, the better way would be to have his treatment follow pretty closely the treatment he would prescribe for diphtheria, so you can understand how he has been at sea with regard to this disease. When he thought he was treating possibly a case of tonsillitis, the patient would die, and that would be the first intimation that the physician received that the patient was suffering from diphtheria. Of course, that was very unfortunate for the case and for the physician as well.

When a case of diphtheria appears it commonly presents itself as a simple sore throat. Usually for true diphtheria, uncomplicated with any local inflammatory condition, the symptoms are not very well marked. Even children, who are apt to have it pretty severely, will make little or no complaint in its earlier stages, and adults will treat the case as just a simple sore throat, whereas in many of the less dangerous diseases of the throat, especially follicular tonsillitis, the pain and distress occasioned by swallowing are so great that the patient's alarm is proportionately great, and this peculiar condition has to be borne in mind in looking for the symptoms. Of course, the temperature and pulse are also to be considered, but here again we find another difficulty. In true diphtheria we find a rise of temperature of not over 101° or 102° F., whereas in a simple follicular tonsillitis, occurring in some people every fall, the temperature will run up to 103° or 104°, or even 105°, giving alarm to the patient and the physician. This is one of the points of distinguishing between the malignant disease and the diseases which are not dangerous. If, in looking in upon the throat, the fauces are found to be more or less reddened, the tonsils possibly bulging out and excreting a certain amount of fungus, and there is generally a great deal of inflammation and corresponding distress to the patient, we may feel, from the clinical examination, that we are dealing with a case of simple pharyngitis; but, however trifling the inflammation may appear, if we discover in connection with it a slimy white patch, with a firm consistency, whether it be the size of a pinhead or whether it cover the whole of the pharynx, and whether the patient

is much or little distressed, clinically, we are justified in the belief that it is diphtheria. We find a white patch in other oral and throat-diseases, but in those cases the membrane is soft and easily pushed aside. If it is true diphtheria, this false membrane, when removed from the throat forcibly, is found to be a tough, a tenacious, thick, clinging membrane, which has been described as a "leathery patch," which is removed with some difficulty, and leaves a denuded, bleeding membrane behind. This is not the case with the soft membranes which are found in the other diseases which are allied to this.

The symptoms which we look for, then, are more or less inflammation in the throat, more or less rise of temperature, more or less depression of the heart, and the presence of the tough, white membrane. There we have, first and last, the clinical symptoms that are present in cases of diphtheria; but, as I have stated, many of these symptoms being present in other diseases, we have chiefly depended in our diagnosis upon finding the actual typical membrane of diphtheria. We have learned a great deal in the last two or three years, and this knowledge has been brought about by the study of bacteriology, and particularly since Klebs and Löffler, a Frenchman and a German, in their combined investigations, discovered a micro-organism, a bacterium, which was always found when this tough, thick membrane was present, and even when it was not present, when the symptoms were as severe as I have described with regard to the extreme depression of the system. This organism was called the Klebs-Löffler bacillus, and at the present time it is the seeking out and finding of that organism on which we depend for our diagnosis instead of depending upon finding the typical membrane. We have found that it makes up the greater amount of this white membrane, and these bacilli, finding suitable food in the mucous membrane of the throat, become firmly lodged there, thus causing the bleeding which happens when the membrane has been removed. In studying out the manner of growth of these organisms, it is found that oftentimes the patient will exhibit all the constitutional symptoms of the disease and yet the membrane may not form, so we are unable at the present time by examining the throat to determine if the bacillus is present or is absent. If the white membrane is present, we look out for mischief; if it is absent, it is not positive proof that the organism is not there. Neither does the amount of inflammation present assist us, except that we may conclude that those cases which seem to be most alarming clinically are probably not diphtheria. It is not enough that the organism

finds its way into the throat to produce diphtheria, it must also find in the throat an inflamed condition which will cause the exudation of serum and be a suitable nutrient media, an agreeable soil on which it can grow. If it falls upon a healthy throat, it is like grains—oats, wheat, or other grains—which fall upon rocks, they will not grow, even under the most favorable climatic conditions, whereas if they fell upon suitable soil each grain would duplicate itself many hundred times. So it is with these organisms. While they may be present in the mouth, they will not multiply unless the conditions are favorable, and it is because of this that many persons who are not suffering from diphtheria themselves go about disseminating these organisms in the various methods, of which I will speak later, causing the mischief which we call epidemic.

Now, the ways in which this disease can be transmitted are very numerous. Probably the most common way is by means of a common drinking-cup, or using cups and glasses that others have used. In talking to you, I am proceeding on the assumption that you all believe that the organism may produce the disease, and therefore shall not go into the details of the bacteriological investigations any further than to say that the fact that some diseases may be transmitted from one person to another is proved by showing that the bacillus can be taken from the body, can be grown in a suitable media, and can be reproduced in animals or other persons, with all the constitutional symptoms of the original disease. This, of course, it is possible to do with diphtheria. As I say, this organism in a healthy throat does not produce the disease always, but is very liable to in a throat where there is any inflammation, and it can easily be seen that the drinking-cup is one of the most direct means of transmission from one individual to another; and this has led, as you have probably learned, in several churches to the adoption in the communion service of individual drinking-cups instead of the common drinking-cup. There is a great deal of opposition to this, many claiming that it is not a communion unless from the same cup; but the claim goes back that the pitcher from which it is poured is the common cup, consequently the distinction cannot hold. The method, which was first adopted in Brooklyn, I believe, and is now used in some of the New York churches, as well as other churches through the country, is to place these individual cups on trays holding say twenty or thirty, and these are passed about among the congregation with great celerity, and the whole practice is carried on more quickly than by using the common cup. As an illustration of the effect of the use of the common cup method, a

case is cited of a lady in Providence who was prevented from participation in the communion service each Sunday, to her very great regret, by the fact that there sat in a pew just in front of her an old gentleman who had a carcinoma of the lip, and she could not bring herself to drink from the cup which was offered to her directly after he had used it; so really the use of the individual cup would bring her perhaps in closer communion with the congregation than the other method. Other means of communication are familiar to the health departments of the various cities, especially among children. One reason, perhaps, why the disease is more common among children is that their playthings are often used in common. You can readily conceive how the use of a fish-horn passed from mouth to mouth may be a means of transmission of various diseases. And then in the public schools, the books and slates and pencils are used in common, and mouthing the pencils is a direct means of transmission. I can give you a good illustration of one of the simple ways by which the disease may be effectually transmitted. I was called to a case of diphtheria, and found a child moribund, or who would be within twelve hours, and the only case of diphtheria in the neighborhood was a child in the same house, who had had proper treatment and had recovered from the disease, so that the case was one which, before the advent of bacteriology, would have been very difficult to explain. The dying one had been away from the house until the child who had been sick was pronounced entirely free from diphtheria and the house had been fumigated, and it was thought there could be no more danger from the disease. Perhaps, if circumstances had not come about as they did, there would have been no danger; but it so happened that the child who had been sick had a ring on its finger which it could not get off. Now, you know, the way to remove a ring that fits tightly on the finger is to suck the finger and moisten it, so in this case number one sucked the finger and did not succeed in getting it off, and then number two tried it, and you can see that the child who was apparently fully recovered from diphtheria was making a direct inoculation from its own throat into the throat of its playmate, and the result was fatal. Other means of transmission among children are the swapping of stick candies and the generous division of chewing-gum with their playfellows; in fact, the conditions are favorable with children for the transmission of the disease. With older persons there are not so many chances of contracting it, and in a great many cases it is difficult to trace it, the patient not having been anywhere where there was diphtheria, and it can only be accounted

for by the theory that the bacilli must have been present on some food utensil, and getting into the throat, a proper soil for its growth was found, and the organism reduplicated to the extent of producing the disease.

This organism is a rod-shaped affair, one millimetre in size, and will grow on almost any media composed of animal material, but it does not grow readily upon dirt or other material outside of the body. It grows readily in bouillon, such as is served for soups, with peptones and salt added, and if we add sugar to that it grows more readily; but if we mix one part of bouillon and three parts of blood-serum we have delightful culture media, upon which this organism will grow with great celerity, particularly so if it is kept at the ordinary temperature of the human body, which is about 35° to 37° C.

In order to put the knowledge that we have to practical use, the health departments of various cities—New York being the first to adopt the method, Washington, Denver, Providence, and many other cities following—have taken up this method of diagnosing the presence of diphtheria by means of finding this particular organism in the throat. Of course, to do this with the patients all over the city, with the large number of physicians attending them, would seem to be impracticable; it would seem as if the technical knowledge required of the physicians for the manufacture and growth of this organism would not permit of its being carried into practical use, but the system has been so nicely developed that any physician who can succeed in seeing the back of a throat will be able to do as well as some other physicians in this matter of diagnosis. In order that physicians may have facilities for growing the bacteria of diphtheria, the boards of health in many cities have caused to be placed in drug-stores or other places in convenient localities this simple arrangement, which I will show you, where the physician may call and get it and use it according to the instructions whenever he wishes to make certain of his diagnosis in the case of a sore throat. It consists of an ordinary slate-pencil box, such as the children have, simple in construction, which is wrapped in waxed paper for two purposes, one being to prevent the evaporation of the moisture which is with the serum, and the second is to prevent, if possible, the contamination which is contained in the box being disseminated to the messenger or to any one else touching the box. The box contains one tube of this prepared serum, which is composed of two-thirds serum and one-third bouillon. After this has been prepared it is placed in a steam sterilizer. That

kills all organisms that we do not desire to grow, because when we come to make an examination of our culture we want to be sure that everything there came from the throat. The other organisms which are liable to be there if this precaution is not taken are the ordinary bacteria of decomposition which will be found in the slaughter-houses and wherever animal tissue is decaying. In order to get the growth on this serum, the circular of instruction directs that we shall take the swab, consisting of a piece of ordinary iron wire, around which is placed some absorbent cotton, and that swab is used for the purpose of swabbing the back of the throat, giving it a circular twist over the whole of the fauces and wherever there is any of this slimy material. A portion of that is caught on the swab, and the swab is then run lightly over the surface of serum. The swab and serum are then replaced in the box, which is carefully wrapped up and sent to the office of the board of health, together with the name of the patient from whom it was taken and the physician who made the examination. Proper record is there made, and the box is again sent to the bacteriologist, who is to grow the culture and make the examination and decide whether the germ of diphtheria is there or not.

To go back to the serum which the physician has sent in: it is placed in the incubator, which is kept at a temperature of 37° C. day and night, constantly, and under these conditions of a suitable media at the right temperature the organisms thrive and grow, and the result is that on the following morning there will be upon the surface of the serum some sort of a growth, some organisms, if the procedures have been carried out as directed, and that growth under a small, low-power lens will manifest itself in the form of little round dots. These dots are what we call colonies,—that is, wherever an organism has been placed on the serum it has gone to work and reduplicated itself and introduced a new colony. As it is estimated that the growth from one organism will amount to about a million in twenty-four hours, you would readily suppose that the colonies would be very large, and if they should continue to grow at that rate we would soon be forced off the face of the earth; but Nature has provided that there shall be a limit to this reduplication. Of course, if we congregate in rooms that are too small, we soon die from the effects of it, and so it is with these organisms,—when they grow faster than Nature desires, they kill each other out and the growth is checked.

The question now before us, when we find these little colonies on the serum, is, What have we here? Is it a benign bacillus or a

malignant one,—a disease organism or an ordinary one? Taking a loop of platinum wire, a small portion of the surface of the serum with these colonies on it is scooped out and rubbed over the surface of an ordinary microscopical cover-glass; it is allowed to dry, and is then stained with a solution of alkaline methyl blue, and the sample is examined microscopically. Then comes the question, which is so important for the patient, for the physician, and for the health department to solve, Are those bacilli of the species known as the Klebs-Löffler? If they are, then the sample is marked "K.-L.," and the result is reported at once by telephone to the physician who sends in the culture, in order that he may avail himself of that knowledge in treating his patient, and for the purpose of using antitoxin if he so desires.

If we do not find the Klebs-Löffler bacillus, it is regarded as a favorable sign for the patient, but not a decisive one. There are several reasons why the diphtheritic germ may not show in the culture, even though it was really present in the throat. The physician in the case may not have reached the back of the throat; he may have contaminated the swab with some other organisms, whose growth may have prevented the growth of the Klebs-Löffler bacilli; he may have failed to rub the swab properly on the serum; so that while we consider that the finding of the Klebs-Löffler bacillus is positive evidence that the patient has diphtheria, yet we do not consider that the absence of that organism is positive evidence that the patient has not diphtheria, and many times a second trial will give us a pure culture of the organism that we are looking for.

Having determined that we have a case of diphtheria, our next step is to at once, without going into the detail any further, notify the local board of health, which immediately, through its inspector, proceeds to placard the house,—that is, to put a card on the door stating that there is diphtheria in the house,—and also to distribute circulars of instruction to the family of the patient and any others who may be in the house. There was formerly great objections to this,—it was supposed to be a stigma on the family; it warned people not to go near them, for they were dangerous; they felt that they had lost their social standing and that they were looked down upon by their neighbors. This objection we have succeeded in partly overcoming by diplomatic means,—by telling them that in permitting this card to be placed there they are protecting not only the public, but are protecting themselves from trouble. For instance, if the card is on the house, any one coming in there does so

on their own responsibility, and they have no redress if they contract the disease; if the card is not on the house, a person who may have had occasion to visit the house and afterwards contracted the disease at some bargain-counter or in the street-car or in a restaurant may bring suit against the parties who had refused to keep the card in sight when it was known that there was a case of diphtheria in the house. The result of this argument is that the majority of people now prefer the protection instead of objecting to what they used to consider a restriction. I regret to say that it is not only the lay people who give us trouble in this respect, but there are many physicians who have not yet seen fit to accept these facts regarding the infection and progress of the disease. They insist that there is no diphtheria there if they do not find the false membrane. The effect of their taking this position has given a great deal of trouble; in the city of Providence four lawsuits have been brought against the authorities as a result of having placed placards on houses where the physician in attendance had declared that there was no diphtheria, he depending entirely on finding the white membrane. The health department depends entirely upon the culture, and if that shows that there are organisms there in the throat, even though the membrane may have disappeared, the card remains, and as a result there is contention by law,—in other words, it is science against ignorance,—which will have to be fought out before intelligent courts. We have in the hospital now a case which is an excellent example of this absence of the clinical evidence of the disease. The membrane has long since disappeared, but we have taken ten or twelve cultures from that throat, extending over a period of eight or nine weeks, and we have found the germ in every one, while others in the same ward have come and gone within the same period. There is a case on record in the Boston Hospital of a lady from whom it was very difficult to expel these organisms; she continued to have them from early in May until late in October. You can readily see what a source of danger such a person would be to the public if she went about as if in perfect health. There is very little fear of infection if the patient is at the hospital or is properly quarantined at home, except to the nurse. It is the time when they get well,—that is, when they feel well and no longer suffer from the constitutional effects of the disease, and they go about among people,—that is the time when they are more dangerous to the general public than when they are critically ill themselves.

Now, the peculiar feature of this disease, as well as of other dis-

eases caused by various kinds of bacteria, is the fact that it is not the bacillus itself which does all this harm, but in the process of its growth it produces a poison, a toxin, and this toxin, being absorbed by the system, produces constitutional effects to the extent of causing death; or, on the other hand, to the extent of the system finally becoming able to resist the action of the toxin. Immediately upon this toxin taking effect upon a patient, an antitoxin begins to form, which is quite antagonistic to the growth of the organism and to the formation of the toxin; the constitution being strong and capable of resisting the action of the toxin until a sufficient amount of this antitoxin has been formed, the throat of the patient, which before was a very desirable soil for habitation and reproduction of the bacilli, now becomes distasteful to them, and the reproduction ceases. On the other hand, if the system is not able to resist the effects of this toxin, the reproduction goes on, and results in the death of the patient. Now, making use of that knowledge that we have of the growth and effects of this bacterium, we set about to see if we cannot find some means of counteracting it, something which we can use for the treatment of this disease. That is done by a series of experiments progressing from laboratory to animals and from animals to man. We take the organisms which we know to be pure diphtheria and put them into a bouillon media, and allow them to remain in an incubator for forty-eight hours, during which time a rapid reproduction goes on, and taking a certain portion of this culture, we inject it into a horse. We could obtain the same results from other animals, but the horse is used because it is larger, stronger, gives more serum, and is less prone to die during the operation. Within twenty-four hours after the culture is injected he has a slight chill and a slight fever. When he recovers from this, we introduce double the quantity of toxin, and go on repeating this until we can introduce quite a large amount of the toxin without producing this fever, and this is a matter not of one or two days, but of six, seven, or eight months. The horse at that time has produced within the blood a quality which neutralizes the effect of that particular toxin whenever introduced. We then consider him as immunized, and his blood has within it the antitoxin quality which is so antagonistic to the development of this bacterium; in other words, they refuse to grow on suitable soil if there is this quality in the blood. Our next step, then, is to extract the blood from the horse which has been thus immunized, under antiseptic precautions, of course, by tapping the jugular vein and drawing off one or two gallons at a time. This is allowed to stand in the ice-

chest until the clot is formed, and the serum is then combined with a suitable preservative camphor, and carbolic acid seems to serve the purpose very nicely. Now, taking this material, it is used on smaller animals for the purpose of ascertaining its strength. Various animals may be used, but the guinea-pig seems to be the most serviceable for bacteriological work. If we inject the toxic material into the guinea-pig, he dies in twenty-four hours. If, however, after the toxin has taken effect, we inject the antitoxin, we find that the guinea-pig recovers. The question now is, Where do we get the balance? How do we know how much antitoxin is going to be required to neutralize a certain amount of toxin? What is the unit from which we can proceed? Of course, it has taken a great deal of investigation to learn that one-tenth of a cubic centimetre of antitoxin would neutralize the effect of a cubic centimetre of the toxin,—that is, the antitoxin serum neutralizes ten times its strength in toxin. But the antitoxin seems to have the effect of stimulating the system in such a manner as to check the growth of the organisms. It gives assistance to the leucocytes, which are the large, round, white cells which are in the blood, and which seem to have a police protection over the body. These leucocytes have the power of resisting whatever poisons may have entered or generated, whether chemical or bacterial; they endeavor to overcome the effects of these poisons and to expel them from the system. So long as the leucocytes have the power to resist or dispose of the harmful organisms, so long are we able to resist disease; but if the whole system is depressed, then the leucocytes do not respond so quickly and vigorously, and these organisms of various kinds gain a firm foothold, and the toxin which is given off during the process of their growth is absorbed into the system. If we succeed in getting in the antitoxin soon enough, before the nervous system has been saturated with the toxin, we assist these leucocytes in their battle with the organisms, and the patient becomes an uncongenial place for their growth.

We may be asked, How do you know that this antitoxin is good for the patient? We reply, Statistics have shown this to be the fact. But statistics may be used in various ways. The figures of reliable observers show that the mortality, which was perhaps thirty to thirty-five per cent. of the total number of cases before the use of antitoxin, has decreased, until it is now claimed that but about seven per cent. of deaths occur where antitoxin is used. The argument is also raised by the other side that it is not a virulent epidemic; that under certain climatic conditions the organism has

a more severe action. Personally, I do not rely so much upon statistics as I do upon actual observation, for when you are called to your patient, you find the trachea obstinately producing a whistling sound, gasping for breath, the throat tumefied to the extent that it almost prohibits the passage of air to the lungs, and covered with excretions, with that dreaded membrane, and in one night after the injection of the antitoxin the changes occur, and on your next visit the patient may be respiring freely, the slimy material has disappeared, and the tumefaction has subsided, leaving usually that white, tenacious, leathery, tough membrane standing out in its true character. This we consider the most effective argument for our present system of diagnosis and treatment of diphtheria, and where it is of especial value to us is in determining the character of those cases where there are no symptoms present, the liability being so great in such cases of transmitting the disease to others. The importance of this was never understood until recently. After a case of diphtheria had recovered, we went through the usual process of fumigating, and thought we had completely eradicated the disease from that house; but in a couple of weeks we had another case in the same house, and we could not understand how it happened. We have now followed the matter up, so that the law of the city of Providence requires that when a child has had diphtheria and desires to return to school, it shall be shown that there are no diphtheria bacilli in the child's throat, nor in the throat of any member of the family. It has entailed lots of work, but it has taught us a great deal. We have a pretty good control of the disease when patients are admitted to the hospital. They are not allowed to go from there until we have obtained negative cultures on three consecutive examinations. That places us in a very certain position with regard to the patients' condition when they leave the hospital, and our experience in this matter has shown the value of this precaution, for of the number of cases examined as precautionary about twelve per cent. showed the presence of the diphtheria bacillus for several days after the patients appeared to be fully recovered. Now, if that twelve per cent. had been allowed to go free, it is impossible to estimate the number of cases which might have resulted, though the original patients would probably not have succumbed to the disease.

In the use of the antitoxin the method which is employed is to use a quantity which we think is equal in proportion to the weight of the person requiring it, and that amount runs all the way from one thousand to two thousand antitoxin units. The usual method

of introduction is by means of an ordinary syringe, such as is used for drawing off pus from a cavity, and the cylinder used to be capable of containing about five cubic centimetres; but now they have succeeded in immunizing horses to such an extent that two cubic centimetres of serum will do what ten used to do. The place selected is usually a place where a fold of the skin can be caught up between the thumb and forefinger,—by some, between the scapulæ on the back; by some, on the thigh. The place which I select is the side of the abdomen just below the ribs. The skin here is less tense and can be easily grasped, and if there is a movement of the patient, less motion is found than nearer the extremities. Though the pain to the patient is nothing more than would result from a pin-prick, there is usually a good deal of scratching and struggling and sometimes opposition on the part of the family, by whom it is thought that the injecting of something under the skin must be more dangerous than putting things down the throat.

Now, one other point: In regard to the question of what is to be done, after the patient gets well, with the clothing, furniture, eating utensils, handkerchiefs, and anything in the room which may have received the bacillus from the patient. Of course, it is quite a puzzle to know what to do so that we may feel certain that the room and articles used by the patient may be used by other people without danger of contracting the disease. All the clothing, bedding, and anything removable are usually put in a steam sterilizer, and for the nicer articles we now have hot-air cylinders, where these articles are allowed to remain for one to two hours at a very high temperature,—say 250° C.,—and this intense heat will kill all germ-life, but does not injure the articles. A fine laundered shirt may be put in there, and it will come out in as good condition as it went in. But the conditions in the room, of course, still remain, and how to get into every nook and crevice of that room and make sure that we have destroyed all the bacteria has always been a source of anxiety to us. We used to go into a room with a pound of sulphur for every thousand cubic feet of space and set fire to it, and think we had done considerable, but we learned through our investigations in bacteriology that we had really accomplished very little, for, upon taking a culture of the diphtheria bacilli which had been exposed to the fumes of the sulphur the usual length of time, we found that it could grow, showing that the sulphur had no effect whatever. In addition to that, the sulphurous acid would tarnish everything unlacquered in the room and destroy the clothing, if moist. Chlorine is of no value, because it rots everything,

so that for a time we were obliged to do our house-cleaning with bichloride, a solution of, say, one five-hundredth, and do the best we could; but we have recently discovered, again through the work of bacteriologists, a germicide which will destroy not only diphtheria organisms, but every other organism of disease at present known. It is by means of a simple flame from wood alcohol which passes through a sieve made of asbestos and platinum. The vapor of the wood alcohol, passing through a heated sieve, produces formaldehyde, and that, escaping into the room in certain quantities, will saturate every article there and penetrate into every crack or corner; but it must be introduced into the room in a rapid manner and in sufficient quantity, when it succeeds in killing the organisms after an exposure of four or five hours, and upon that we shall rely, probably, in the future for sterilizing, until some more effective germicide is discovered.

You must pardon me for taking up so much of your time. I hope I have not destroyed your confidence in selecting a speaker not exactly in your own profession and a subject not exactly in your line, but, as I said, it is a subject which is fraught with great interest not only to the health department but to the entire community, and I am very glad to find that you have taken an interest in it.

DISCUSSION.

Chairman Cooke.—I am sure we have all enjoyed the interesting talk which our brother has given us, and the subject is now open for discussion by the members. If there are any points not fully understood by the members, I am sure Dr. Swarts will be pleased to answer any questions.

Dr. Werner.—I would like a repetition of the last part of his address, where he spoke of the disinfecting method that is in use now. I do not think I quite understood it.

Dr. Swarts.—Let us take, for instance, a tin,—any ordinary tin, such as a pan for setting out milk,—pack it with asbestos, and pour in some ordinary wood alcohol and ignite it, and the fumes coming up are caught by means of a circular tin which is made to fit over the base, and resembles somewhat a stove-pipe. About half-way up this circular tin let us insert a diaphragm of asbestos, which has little holes in it. That asbestos has been platinized, and is in such a condition that it can be brought up to a white heat without doing it any injury. Now, after that diaphragm has been heated, the alcohol flame is extinguished by placing a cover over the top of the

circular tin for a moment; then the vapor of the alcohol in passing through the heated diaphragm becomes formaldehyde. It is wood alcohol up to the heated diaphragm, beyond that it is formaldehyde gas. The value of this gas as a disinfectant has been known for some time, but it has not been possible for us to make practical use of the knowledge, on account of the great expense attending the manufacture of it, the cost of the solutions for its production previous to the perfection of this simple apparatus which I have explained being something like two to three dollars a pound. It is very effectual. You may place some of these diphtheria bacilli, or any other organisms, in any part of a room,—under the carpet, in the bed, in the closets, or anywhere you choose,—and when the formaldehyde is generated in that room it will be impossible to protect them from it. It will penetrate any kind of cloth, as well as the wall-paper, and even wood. In whatever part of the room the organisms may have lodged, it will find them and destroy them.

Dr. Werner.—Does it do any harm to the room or anything in it?

Dr. Swarts.—None whatever. It is a dry gas which apparently has no effect on any substance in the room.

Of course, the point we have been after most in this question of disinfection is the destruction of the tubercle bacillus. You know that the railroads carry a great many people West and South who are in different stages of consumption. The Pullman cars in which these patients ride are thoroughly cleaned every time they come in, say once a week, but, of course, it would be impracticable to use the ordinary method of fumigating. The method which is commonly used by them is to force a powerful blast of air through the upholstery and cushions, by which means the dust is freely blown out of the seats or the corners of the car in which it may have accumulated. They have to use very powerful air-pumps, and you will naturally see that the railroads have spent a great deal of money in trying to find something to kill these organisms without injury to the cars. When this method of producing formaldehyde was perfected, the investigators sent to the Pullman Car Company and asked them for every kind of cloth which was used in the manufacture of their cars. They sent on some two or three hundred different patterns and colors of cloth, silks, satins, brocades, and the various cloths which they used, and the formaldehyde had no effect whatever upon them, except in two instances, where it affected the colors. I think one was a shade of blue and the other a shade of brown. The fabrics were not injured in the least. This formaldehyde will produce an odor, but by placing a given quantity

of ammonia in the room the air there will be almost as pure as ozone within an hour or two.

The introduction of this method of disinfection is certainly the beginning of the happy era for the inspector of the boards of health whose duty it is to fumigate houses after the recovery or removal of cases of diphtheria and other diseases. He used to be looked upon as the *bête noire* of every one he came to visit. Everything about the house was turned topsy-turvy, while many utensils and ornamental articles were completely ruined. We are not obliged now to take our clothing to the steam sterilizer, for the inspectors know when they have killed every germ in the room. For instance, in testing, their method is to take a piece of cloth and saturate it with a bouillon solution of a culture of some kind of bacteria. That is wrapped up in several thicknesses of cloth and placed between the mattresses of a bed; the inspector then goes through the usual process, and on his return he removes the package containing the bacteria culture, which is sent to the board of health, and a microscopical examination shows them to be dead.

Chairman Cooke.—How much expense and how much time does this process require in the case of the Pullman cars?

Dr. Swarts.—The expense would not be over sixty or seventy cents for a car, and the labor would be that of a porter. It should be allowed to continue for, say, ten hours, although we know we can destroy them in four to five hours, and if a car was obliged to start out again in that time it would be in good condition. It is the custom to shut it up over night, and there is then no doubt that it is absolutely sterile.

Dr. Bradley.—I would like to ask if, as this remedy has been shown to be so destructive to bacteria, some preparation of it could not be used in the treatment of the disease itself? If it will prevent the germination of the bacilli and destroy them, could it not be used in some form to help the patient to resist or overcome the disease, the same as the antitoxin?

Dr. Swarts.—I have thought a great deal on that subject, and several persons are still working upon it. The trouble we meet with is that the treatment necessary to kill bacteria is injurious to tissue.

Dr. Bradley.—Is a patient who has once been affected by the disease, diphtheria, more subject to it than another person? My own son, when he was three or four years old, had quite a severe attack, and after he was pronounced fully recovered he had another slight attack. I never understood whether the second attack was

one of those purely accidental cases or whether the fact of his having had the disease before rendered him more subject to it.

Dr. Swarts.—It does not, so far as we know, except it may reduce the general constitution to a point from which the patient may never recover.

Dr. Bradley.—How long is the immunity of the average person after recovery from a case of diphtheria?

Dr. Swarts.—That is a question which cannot be satisfactorily answered. We cannot say, as in typhoid, that it may continue for life. It is generally believed to be six months to a year, though cases have been known where persons have contracted diphtheria within four months from the time of their recovery from a previous attack. So, you see, it does not come under the head of those diseases which the old ladies claim it is better to have and be over with. You know, when they used to hear of a case of measles or mumps, they used to say, "It's a good thing for the children to have; let's go and get it." It would not be wise for a person to deliberately contract diphtheria.

Chairman Cooke.—Take it in this patient that you spoke of who went from May to October. I do not think you stated whether the case was fatal.

Dr. Swarts.—Oh, no, the patient was immunized. The organisms could not overcome the antitoxin quality in her blood, and, at the same time, in this case the organisms refused to be expelled from her system. As far as she was concerned, they could do her no harm. She was perfectly free from the disease, and could have gone about wherever she chose, and thereby would have been a greater source of danger than a person who was seriously ill with the disease, but who was confined to one room with only a doctor and a nurse.

Dr. Clapp.—How long after the infection is antitoxin available?

Dr. Swarts.—Before the nervous system has become saturated with the toxin of the diphtheritic germ. It is wholly a matter of the strength of the patient's system to resist the effects of this toxin. If the disease is brought to our attention in its early stages and we introduce the antitoxin, then we assist the system in its efforts, and thereby go on to the extent of immunizing the patient.

Dr. Clapp.—Then there is always hope from the effect of this unless the patient is very much depleted?

Dr. Swarts.—Unless the toxin has become distributed through the system to such an extent that it is beyond the power of the system, aided by the antitoxin, to overcome it. The horses which

are used for the manufacture of antitoxin will live for an indefinite period if they do not succumb to the toxin during the process of immunization, a process which, as I have stated, extends sometimes over a period of eight months. They then proceed to draw from the horse, at the same time continuing the injection of the toxin, thereby keeping up the strength of the antitoxin quality of the blood to the standard as measured by its effect on guinea-pigs of certain weight. Some horses will not yield serum up to the standard strength, and when they find that a horse refuses to do so they sell him and get another horse. Occasionally they find one that is remarkable for the amount and quality of the serum produced. There is one old white horse in New York which produces great quantities of serum of excellent quality.

Chairman Cooke.—They milk them, same as you would a cow?

Dr. Swarts.—It has the same effect, but you get at it by a different process. The process is not a very painful one for the horses; they do not mind it.

Chairman Cooke.—Where are the most reliable preparations made?

Dr. Swarts.—Antitoxin is now made in several places in this country. Perhaps Mulford & Co., in Philadelphia, have paid the most attention to its manufacture. They are a druggist firm, and took up this question of antitoxin as soon as it came out, and are carrying on a perfectly clean business in every way. It is made for the city of Boston somewhere in the environs of Boston,—I do not remember now just where,—and by the State Board of Health of Massachusetts. The examinations of cultures sent in by physicians are made by the State, free of expense; the antitoxin is also given free to those patients who cannot afford it. At the office of the board of health we keep the antitoxin and syringes always on hand, and our instructions to physicians are, when they find a case of diphtheria, where the patient is too poor to buy what is necessary, to send at once to us and borrow the syringes and get a sufficient quantity of antitoxin, and use it quickly.

Chairman Cooke.—Any special method of cleaning those syringes after they have been used?

Dr. Swarts.—No; five per cent. carbolic acid will be sufficient. There would not be so much danger from the blood of the patient on the needle as from dirt which might be on the patient's skin.

I have brought down with me to-night a stained specimen of the Klebs-Löffler bacillus, which I will place in the microscope for your examination.

Dr. Eames.—I wish to express my appreciation of the manner in which the subject has been handled. I have been quite familiar with these methods and know something of the work which has been done in Providence, Rhode Island, from a friend of mine in that city, and I can say truthfully that I have been pleased and agreeably surprised to find my attention held and my ideas thus stimulated. I think we all must now have a better understanding of this subject.

The mention of individual communion-cups interested me, for the idea was original with myself and resulted in the first use of them in New England. I believe that the discussion of this question will show that diseases may be transmitted from sources which formerly we had no idea were means of transmitting and propagating disease, and that the study of bacteriology will enlighten us still further in regard to this matter. I am surprised that men in the medical profession still avow that the common communion-cup is not a source of danger.

Another point which should be emphasized is the fact that there are two forms of stomatitis which may be confounded with diphtheria,—the aphthous form, also that form which is produced by means of parasites, stomatitis mycosa. In both of these we have a white patch, especially in the early stages of the aphthous form, and it should be pointed out that in aphthous stomatitis the white spots appear on the inner surfaces of the lips, on the gums, and on the edges of the tongue, while in diphtheria, as has been pointed out, the white patches are found on the tonsils and on the edges of the palate, extending from behind forward. The tough, leathery, diphtheritic patch is in contrast to the softer consistency of the aphthous patch, which is easily detachable. The sore throat and more profound systemic disturbances of diphtheria are not present in stomatitis, as a rule.

Dr. Werner.—I, for one, appreciate the scientific, concise, non-repeating talk of this evening, and I am very glad I came. It is a pleasure to listen to one who can talk directly to a subject. Dr. Swarts has that faculty to an extent which I have seldom heard in speakers. It is something we all ought to learn; we have certainly had a profitable lesson in the address to-night. The method of disinfecting with wood alcohol and asbestos is of value to know.

Dr. Eames and others have spoken of communion-cups. Just compare for a moment the number of people who go to communion with the number of school children who drink in common from dirty cups. We ought all to support in an effective way any move-

ment to improve the conditions existing in our school-houses; we ought to attend meetings which give expression to the care of text-books, drinking-cups, pencils, etc., for the school is a decidedly more important place to begin our reform than the church.

Dr. Potter.—I cannot help saying a word about what has been something of a hobby with me for a good many years. It is undoubtedly a wise thing to agitate this matter of communion-cups and common drinking-cups, but we must not overlook our own instruments and the apparatus we are constantly using. I do not wish to go into particulars, but simply to urge the importance of knowing that our instruments are sterilized by a reliable process after every use.

Dr. Swarts.—That point—that the instrument may be a means of infection—I did not see fit to dwell upon, as I supposed that you had heard a great deal about it. I am glad, however, that it has been brought up, as it is right in line with something that I did intend to speak of.

In the patients coming to you for dental operations, should you see a throat that is intensely inflamed, with little dots on the tonsils, or any extended inflammation of the mucous membrane, it would be policy to discontinue work on that patient, for two reasons: first, you are running the risk of contracting diphtheria through the patient coughing in your face or infecting yourself by the handling of the patient; in the second place, the patient should be at home and receiving medical treatment, not only for his own sake, but for the good of the public; and by advising him in this matter and notifying the board of health, with the consent of the patient, you will be doing both the patient and the health department a service. I say "with the consent of the patient," for if you do not obtain his consent you are likely to get into trouble, or, at least, forfeit his friendship. I, unfortunately, as a physician, was called into a house where there had been a death in the family from diphtheria to make examinations of the remaining members. I told the lady that if I was to act in the capacity of physician I should be obliged to follow the procedures prescribed by law. She insisted upon my being the physician, and though only one of the family was really clinically sick, yet examination of the cultures in that family of four showed the Klebs-Löffler bacillus present in two persons. I followed the legal requirements, and three hours afterwards was dishonorably discharged. As a physician, I reported to the health department the examinations made, accompanied by the cultures which I obtained; as a bacteriologist, I

reported to the health department the discovery of the Klebs-Löffler bacillus. The superintendent of the city health department ordered the patients quarantined and the house placarded in the usual manner. Another physician was called in, who declared that the patients were entirely free from diphtheria, and as a result there is now a five-thousand-dollar lawsuit hanging over my head for malpractice. So get the consent of the patient before you take any steps in a matter of this kind; but it would be a great help to the health department in this city, and in all cities, if cases of this kind were looked after at the earliest moment possible, because certain isolated cases might be the means of transmitting the disease to such an extent as to cause an epidemic.

Dr. Clapp.—To what extent is the quarantine carried?

Dr. Swarts.—Our practice in Providence is to quarantine the patient with one attendant, and, so far as practicable, to confine them to not more than two rooms. Of course, it is often difficult to do this, especially when your case is in a tenement house of, say, a half-dozen rooms, and there are seven children and three adults living there. We then insist that no children in that family shall attend school or Sunday-school, and that a watch be kept over them to prevent them from playing with other children. The father of the family is allowed to continue at work, and he also is instructed as to his relations with his family and with other people. The proper thing to do in a case of diphtheria would be the quarantining of every one in the house, if necessary, feeding the family at the public expense, and paying the bread-winner his customary wages, and I think that eventually this will be done. It is going to be expensive, but when compared with the cost of treating the cases it is more economical.

THE regular monthly meeting of the American Academy of Dental Science was held at Young's Hotel, Wednesday evening, May 5, 1897, at six o'clock, President Andrews in the chair.

Subject for discussion was "Replantation of Teeth."

Dr. Perrin.—The operation of replantation, though rarely required, is one that should be familiar to every dentist, for occasionally a case presents itself in which no other treatment will effect so rapid a cure.

I have had several cases where replantation was resorted to, with such happy results that I feel prompted to state them at this time. In each of these pericementitis was present to such a degree as to cause unbearable pain; removal of the filling or perforation of the tooth was impossible. My method has been, after extracting the tooth, to remove the filling, cleanse and disinfect the pulp-cavity and root-canals, and fill the same with the usual materials. I also remove the inflamed portion of the pericementum and grind away about one-sixteenth of the apex of the root. The socket is then washed out with warm water, being careful to remove all clotted blood. The tooth is then pressed firmly into place, and perfect adaptation results in about a week.

My first case was in May, 1884. A man about thirty five years of age, with a left superior bicuspid affected with periodontitis. The tooth was extracted and replaced, and has served well for over ten years. My second case was in November, 1886, also a left superior bicuspid, very sore to the touch, and containing a large amalgam filling, the removal of which was impossible. The tooth was extracted and replaced, and in less than a week was as useful as ever. It is still perfectly firm and useful.

CASE III.—December, 1890. A young man twenty-two years old. First right inferior molar in same condition as the preceding cases. This was extracted and replaced, and, although it was necessary to grind the roots quite extensively to make them parallel, the tooth soon became firm and healthy, and is now in good condition.

CASE IV.—November, 1892. A lady brought in her hand her second left superior molar carefully tied up in paper, it having been extracted by mistake while removing the third molar. This was replaced, and is to-day a useful tooth.

President Andrews.—How many cases have you had in all?

Dr. Perrin.—Only these four that I have record of. I have had several others the results of which are unknown.

Dr. Cutter.—Did you say you simply washed out the socket with warm water?

Dr. Perrin.—Yes; but I am sure to get out all particles of loose tissue and scrape off the reddened pericementum from the root.

Dr. Bradley.—May I ask Dr. Perrin if in any of these cases there was an established fistula?

Dr. Perrin.—In none of these cases had there been an abscess formed; there was simply a condition of pericementitis.

Dr. Stevens.—I had a case where I removed a right central incisor to relieve an alveolar abscess, a blind abscess that was very painful. I removed the tooth, cleansed the socket of the abscess, thoroughly sterilized the socket and the tooth, filled the canal, and returned the tooth to its place. The patient in two days was entirely over the effects of the operation, and the tooth has never given further trouble.

Dr. Bradley.—Did you wire the tooth in place?

Dr. Stevens.—I did, but removed the ligature after two days.

Dr. Allen.—I have had three of these cases altogether. The first was that of a young woman who came to me suffering from a severe chronic alveolar abscess with a fistula over the left superior cuspid. Another dentist had previously treated the tooth, but to be able to work intelligently I removed the filling, and found the canal (which had been filled with gutta-percha) free from odor and perfectly sealed to the apical foramen. After opening the foramen and treating the case several days, I was surprised to find that my patient experienced no relief. I then decided to extract the tooth, to which proposition the patient readily assented. Upon removing the tooth, I discovered the cause of the trouble to be a sharp spur of calculus extending at right angle from the root, at a point fully an eighth of an inch from its apex. I removed the calculus, polished the roughened surface (there being no evidence of necrosis), and returned the tooth to its socket. It was my pleasure to examine the case two weeks ago and to find the tooth apparently as firm as any normal tooth, having stood a three years' test.

Another case was that of a lady sixty years of age. The tooth in this instance was the right inferior sixth-year molar, which I treated in the same manner as described by Dr. Perrin. A year later it was doing well, but I have had no report from the patient of late, though it is likely I should have been informed had any unfavorable symptom developed.

The third case was that of a gentleman for whom I crowned the left superior first bicuspid. In drilling out the canal for the

pin of the crown I accidentally encroached upon the bifurcation of the root; this opening was sealed as well as possible with gutta-percha and the crown adjusted. The tooth remained comfortable for six months or so, when it began to show signs of suppuration. Knowing well the cause of the trouble, I extracted the root and the crown together, filled the perforation in the root with gold, and then replaced the root in its socket. The gentleman is now in Europe, and but recently sent me word that the tooth was still doing good service.

Dr. Adams.—I have had some experience in this line. One case in particular which I remember was that of a lateral incisor which had an alveolar abscess that had been discharging for three years. I could not cure the case, so I extracted the tooth and cut off the end of the root, which was denuded, and returned the tooth to its socket. It remained in a perfectly healthy condition for years, until the death of the patient. The details of the operation were as follows: The root was filled through a cavity in the palatal surface; this cavity was filled with gold, which was built out beyond the surface, and the ligatures which were to secure the tooth in place were embedded in the filling; this was done to prevent the slipping of the ligature.

Several years after the replanting of the tooth a cavity appeared in the proximal surface, and I wedged the tooth apart and filled it without injury to the tooth.

I find that the length of time during which such teeth will serve is variable. Some of them have, at the end of perhaps seven or eight years, suddenly given out, and I would find that the root was badly absorbed and the tooth had gone completely to pieces. But I think in cases of abscesses which are obstinate it is very good treatment.

Dr. Eames.—At a meeting of the Massachusetts Dental Society in 1884 I exhibited a case in which the superior lateral was turned on its axis, so that its mesial surface presented directly forward, and I then explained that I had extracted that lateral, enlarged the socket, and returned the tooth to its proper position in the mouth. I had also another case answering in every particular to the one just described, and treated in the same way. The replanted teeth performed satisfactory service in each case for about ten years, when, as I have been informed, they came out. I thought it rather remarkable that they lasted about the same length of time.

Another case was that of an inferior cuspid, replanted about the year 1877, and which did good service for about fifteen years. Some other cases lasted perhaps not more than six to eight years.

Two or three are still giving satisfaction, with a record behind them of eight years. I will simply refer to two cases of recent interest,—one a superior left central which had had a fistula on the labial surface for a number of years. A crown had been put on by a former dentist, which had been recently removed in order to treat the root, and the present dentist had treated it for months without healing the fistula. I saw the case in consultation, and after making a counter-opening and injecting antiseptic fluids freely without good results, I etherized the patient and made a free incision near the apex of the root, removed the tissues there, and with a large new bur amputated the end of the root and burred away the bone in the vicinity. But the fistula still continued to discharge. The last resort was the plugging of this central root (which was a mere shell) with wood and extracting it. It was found that on the palatal surface of the root was an opening from the canal which had been drilled through, and that filling-material had been stuffed through this opening plentifully. It was in such a position that it would have been impossible to get at it by any reasonable surgical interference without extracting the root. After curetting the surface thoroughly and washing the cavity with biehloride of mercury solution, I prepared the root and returned it to its socket. A plate which was being worn kept it in position, overcoming its tendency to come out. This was done about six weeks ago, and the tooth is now doing very well, being fully as firm in its socket as before being treated.

In another recent case there was violent inflammation with swelling of the face and gum, and a fistula from a superior right cuspid. On account of the great pain the tooth was extracted, and a large sac was found at the end of the root. This was removed with a portion of the root and the tooth returned to its socket. This tooth had a strong tendency to come out; holding it in place for a long time with the fingers was not sufficient to keep it there, I therefore ligated a wire to the second bicuspid and passed it over the cuspid and ligated it to the lateral. The implanted tooth was then tied down to the wire, which held it in place. It now appears to be as serviceable as any tooth.

Dr. Robinson.—I had a case, I think it was in 1873 or 1874. A gentleman came into my office with a tooth that was aching and wanted it extracted. Examination showed a cavity which extended nearly to the pulp. I wanted him to allow me to destroy the pulp and fill the canal, but he was impatient and would not have it done, so I was obliged to extract the tooth. I told him I

thought it was too bad to take out a good tooth like that and break up the arch, and I suggested filling it and replacing it, and this he agreed to. I have seen him within two years, and he said that it was the best tooth in his head. I have had several other cases that turned out very nicely.

President Andrews.—Perhaps Dr. Preston will tell us of his experience in this line.

Dr. Preston.—I have extracted several teeth and filled them and put them back again, but I cannot now recall one that ever amounted to anything. The roots all became rough and looked worm-eaten. I filled one for a young boy away back in 1839. In those days you could not destroy the pulp as you can now, and it was necessary either to extract the tooth and put it back, or keep it out altogether.

Dr. Belyea.—What did you fill the roots with?

Dr. Preston.—I did not fill the roots at all, but filled the external cavity with tin-foil. I found one man who seemed to be well pleased with the result of the operation. I asked him about the tooth several years after replacing it, and he said that he had never had any trouble from it, and if any one wanted to know about such an operation to send them to him. The most peculiar case of replantation I ever heard of was that of a lady for whom I extracted a tooth as far back as 1842. She felt badly about having it extracted, and she afterwards told me that when she got home she took an iron spoon and melted some lead, washed the tooth thoroughly, carefully poured the lead into the cavity and put the tooth back in her head. I do not know how long it lasted her.

Dr. Werner.—We have only heard of favorable cases of replantation; while I could perhaps report two that were favorable, I have distinct recollections of two unfavorable cases. In twenty years' practice I have had perhaps six or seven cases, two of which could perhaps be considered favorable, but two others were lamentable failures. I do not consider my experience to be entirely successful, and only in extreme cases do I consider the operation justifiable. I should not think of replanting a root in a diseased socket. I had a case recently where I thought of replanting, and would have done so if I had felt fairly certain of the result. In resetting a bridge I was obliged to remove one of the roots, a central, which was perhaps one of the best of the supporting teeth, and I wished that I could have felt justified in replanting that root.

President Andrews.—I recall two cases of abscessed teeth, where

I extracted the teeth, carefully filled the roots, dressed them down, and replaced them in their sockets; I noticed that in a year or two they became solidly fixed to the jaws, calcified so solidly that in tapping them they appeared to be a part of the jaw. It seemed as if the periosteal tissue about the teeth had completely calcified, and I think you will find that in most cases of replanted teeth on tapping them they give the impression that they have become united by calcification to the alveolus. In my cases the teeth began to loosen in a few years, and the roots seemed to be nearly absorbed through the action of the cells which we call the osteoblasts. Both cases had a similar history.

THE regular monthly meeting of the American Academy of Dental Science was held at Young's Hotel, Wednesday evening, December 1, 1897, at six o'clock, President Cooke in the chair.

A paper was read by Dr. F. B. Lund, entitled "The Place of Local Anæsthesia in Surgery."

THE PLACE OF LOCAL ANÆSTHESIA IN SURGERY.

BY F. B. LUND, M.D., BOSTON, MASS.[1]

Of the advantages offered by local anæsthesia, as compared with general anæsthesia in a large class of surgical operations, there is no need for me to speak before this assembly. Suffice it to say that early in my experience as a medical student I became so convinced of the value of local anæsthesia by hydrochlorate of cocaine, in enabling me to do away with the nauseating effects of general anæsthesia,—the vomiting, dizziness, delirium, and general weakness,—which are so unpleasantly associated with ether in the minds of many patients, that in case of subsequent operations they sometimes prefer, if the operation is a slight one, to suffer it without anæsthesia rather than be etherized, that I have always been an earnest student of methods of local anæsthesia as applied to surgical practice.

The experiences of eight years in the use of local anæsthesia in minor surgery, for which I have had during the past three years the extensive opportunity furnished by the large out-patient clinic at the Boston City Hospital, have given me such definite views as to the use and abuse of local anæsthesia, the class of cases in which it should be employed, and the technique which is essential to give good results, that I have thought it might not be uninteresting to you, even though I am compelled to confess my entire ignorance of local anæsthesia in dental surgery, to hear what I have found to hold true with regard to its use in a general surgical practice. I have had a certain amount of experience in the use of local anæsthesia in major as well as minor operations, and I think you will agree with me that even in the former it has a distinct and legitimate field.

The various methods of producing local anæsthesia which have been up to the present time employed will be found to divide into

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but two fundamental classes,—first, those which depend upon numbing the part by freezing mixtures and evaporating spray; and, second, those which depend upon bringing some drug having local anæsthetic properties in contact with the terminal filaments of the sensory nerves. Of local anæsthesia by cold it is not necessary to speak, save to say that, although much older than other methods, it has always failed to find an extensive field of usefulness,—first, owing to the transitory nature of its effects, which renders it unavailable for operations requiring more than a few seconds for their performance; and, secondly, owing to the fact that the processes of freezing and thawing, more particularly the latter, are both exceedingly painful, so that, even if we have anæsthesia or absence of pain during an operation, we certainly do not have it either before or after it. Since 1884, when Köller, of New York, then a medical student in Vienna, discovered the local anæsthetic effects of cocaine upon the mucous membranes, that drug and other synthetical compounds which have recently followed it have vastly extended the field for local anæsthesia in surgery.

It was found that, although cocaine was effective upon mucous membranes by direct absorption from their surface, its use upon intact skin required hypodermic injection in order to be effective. The hypodermic injections in the early days were made with strong solutions,—eight to ten per cent.,—such as were used as applications to mucous membranes; and it was soon found that, owing to the toxic properties of the drug, but small amounts could be safely injected, so that only very brief operations could be performed under cocaine, without danger of producing constitutional effects. Occasional reports of cases of poisoning made surgeons chary of the use of cocaine, and when they did use it, so small was the amount of fluid injected that the operative field was seldom fully permeated with the drug, and anæsthesia often failed to appear. The technique employed was to inject a few drops of the strong cocaine solution in the neighborhood of the field of operation, and then wait for the anæsthetic fluid to soak into the surrounding parts. This procedure was modified by Dr. Leonard J. Corning, of New York, who was the first to demonstrate that solutions of cocaine injected into and around the trunk of a nerve produced anæsthesia in the distribution of its terminal filaments.

At this stage of our knowledge of anæsthesia by cocaine, failures were frequent, for the reason that it was impossible to tell just how far the anæsthetic effect of cocaine had spread, and incisions were frequently carried into non-anæsthetized tissue, greatly to the dis-

comfiture of both patient and surgeon. The latter, as a result of a few experiences of this sort, was often led to give up local anæsthesia and pronounce it a failure; and so great was the prejudice against it that many surgeons even now hold that cocaine and other local anæsthetics give at best uncertain results, that operations under them are never perfectly painless, and general anæsthesia is to be preferred in almost all cases.

But those who were not discouraged by their early failures in local anæsthesia went on perfecting the technique,—Landerer in Germany, Reclus in France, and Halsted in this country being notable examples. It was discovered that weaker solutions of cocaine gave as good results as the stronger, if properly injected, and that the employment of one- and two-per-cent. solutions allowed the performance of more extensive operations without the danger of poisoning. It was observed, as the solutions were injected in larger amounts, that the tissue was saturated, distended, rendered œdematous by the fluid, absolute insensibility to pain was produced, and cutting, scraping, or other manipulations could be performed without any untoward sensation on the patient's part.

Reclus was one of the first to show that by injecting a syringeful of a one-per-cent. solution into the skin, and then beginning the next injection within the area of anæsthesia produced by the first one, quite large operative fields could be rendered anæsthetic.

Recently, Schleich, of Berlin, has elaborated the technique of local anæsthesia, and shown that the injection of a solution of salt and other drugs has an anæsthetic effect, which is dependent upon the distention or infiltration of the tissues with the solution. He is unable, however, in practical work to dispense with cocaine in his anæsthetic solution, for the reason that, although the injection of a simple salt solution produces a local anæsthetic area, the process of injection is in itself painful, unless a little cocaine—1 part in 500 or 1 part in 1000—is added to the solution. Two years ago I published the results of my experience with the use of Schleich's solutions of salt with 1 part in 500 or 1 in 1000 of cocaine and a little morphine, and stated that I had found them perfectly satisfactory in the performance of numerous minor operations, and a few major operations in which, for some reason, the employment of a general anæsthetic was contraindicated. Since that time, acting on the theory that if it was necessary to add cocaine to the salt and morphine solutions in order to prevent pain during their injection, the cocaine alone, being a more definite local anæsthetic

than the salt and morphine, ought to be sufficient, I have been employing solutions of cocaine in the strength of 1 in 500 in inflamed tissues, and 1 in 1000 in extensive operations, on non-inflamed tissues, and have found the results to be just as satisfactory as when I employed the more complicated solutions of Schleich. I have, therefore, employed the 1 to 500 and 1 to 1000 solutions of cocaine as a local anæsthetic fluid for routine use in operating under local anæsthesia. Also, during the last twelve months, since the new synthetic compound eucaine has been exploited and advocated as more effective and less toxic than cocaine, I have taken opportunity to try it in a large number of minor operations, and have endeavored to carefully compare it with cocaine. I have found that in the strength of from 1 to 200 to 1 to 500 it had marked local anæsthetic properties, and that it produced anæsthesia which was as complete and about as long continued as that of my cocaine solutions. I have found, however, that in inflamed tissues eucaine solutions are more apt to cause pain during the injection than is the case with cocaine solutions, and that in operations in vascular regions, such as the face, the fact that it causes dilatation of the blood-vessels is a decided disadvantage. As is well known, infiltration with cocaine solutions causes constriction of the smaller blood-vessels, so that during a small operation on the face the surgeon is often not at all hampered by capillary oozing, at least during the first part of the dissection, when he is making his incision or getting down to the capsule of a tumor. When eucaine is employed, the furious bleeding which ensues seriously hampers the surgeon. I have employed a eucaine solution in the removal of a fatty tumor the size of the fist with perfect success in every respect, except that the hemorrhage was, I think, somewhat more violent than it would have been if cocaine had been employed. For injection into the bladder for the purpose of using the cystoscope, I think I should prefer eucaine, as large amounts of a strong cocaine solution, which, under these conditions are required, are apt to be absorbed by the mucous membranes, and cocaine-poisoning might result. I have never had any toxic effects from the use of cocaine in the weak solutions in which I have employed it for infiltration. I see no reason for discarding it in favor of eucaine, and have, therefore, continued to employ it as a routine anæsthetic. Eucaine is somewhat less expensive than cocaine, and it is claimed that solutions of it can be boiled without decomposing, which is not true of cocaine.

Whether cocaine or eucaine were employed, I have found that my success in securing anæsthesia depended entirely on the care-

ful carrying out of a certain technique. Careless technique always results in pain to the patient and reproach to the surgeon. There are certain operations, and some even of a minor character, in which considerable practice has not enabled me to attain sufficient skill in the technique to avoid causing pain, and in these, therefore, unless contraindicated for some special reason, I should advise the employment of a general anæsthetic. The one essential point in local anæsthesia by cocaine or any other solution, and this was well emphasized by Schleich, is the complete infiltration of the tissues with the anæsthetic fluid. A condition of artificial œdema must be produced, so that the sensitive nerve-filaments are thoroughly surrounded by and saturated with the fluid. In proportion as it is possible to produce this condition practically, will we be successful in our results. The fluid must precede the needle, and the needle the knife, and if we never cut or push our needle or other instrument beyond the territory which has been saturated with the anæsthetic fluid, we can operate without pain to our patient. The method is as follows: the skin being, if loose, pinched up between the thumb and forefinger of the left hand, or, if tense, depressed by pressing down obliquely upon it with the needle-point, the point of the needle is slowly introduced, with the opening, which is cut obliquely, it will be remembered, downward until it is partly through the outer layer of the skin. Then a few drops of the solution is forced in, the posterior part of the opening being closed by pressing the needle downward upon the skin. A few drops having been injected, the needle-point is entirely introduced into, but not through, the skin, and a syringeful of fluid injected, making a raised white wheal, looking like a mosquito-bite. Over this wheal it will be found that the skin is perfectly anæsthetic. Then the needle is withdrawn and introduced within, but near the edge of, the anæsthetic area, and more fluid injected, extending the anæsthetic area in the required direction until sufficient skin area has been covered. If it is desired to remove a wen or small subcutaneous tumor, a circle of these wheals is made around it, and then the needle is pushed under the tumor and the tissues beneath it infiltrated. This process, if properly performed, has caused the patient no pain, or, at most, only a slight pain at the first introduction of the needle. Then the tumor may be dissected out, the knife cuts never extending beyond the area rendered œdematous, without the slightest pain to the patient. In deeper operations, requiring the division of muscle, fascia, and periosteum, after the infiltration of the skin and subcutaneous tissue, these may

be cut through, and the fascia and periosteum infiltrated under the guidance of the eye, and then operated upon strictly within the anæsthetized area. In certain conditions this technique is easy, and in others difficult or impossible to carry out, and there are, therefore, certain cases in which we can operate without ether or chloroform and without pain, and others in which we cannot.

Experience with a large number of cases enables one to properly choose those adapted to local anæsthesia, and save his patients the discomfort and danger of general anæsthesia in many operations of minor surgery. It also enables him to avoid the discomfiture which results from promising to operate without pain in cases in which he is unable to carry out his project, and either has to hurt his patient badly or, after the delay caused by an unsuccessful attempt, to have recourse to general anæsthesia.

Any transgression beyond the anæsthetized area will cause pain to the patient, and in operating in inflamed tissues the pulling upon the inflamed area by retractors and the like, even if they be in contact only with anæsthetized tissues, will cause pain and difficulty.

In general, the deeper and more complicated the dissection, the more difficult and irksome will be the carrying out of a satisfactory technique. In some cases, where large nerve-trunks have to be cut across, I have found the infiltration difficult to carry out.

Schleich recommends the anæsthetization of nerves by the application of a drop of strong carbolic acid to their divided ends, but such a procedure certainly does not mitigate the pain of their division, which is severe, and I have found sometimes that I came upon non-anæsthetized nerves in areas which were otherwise satisfactorily infiltrated, and that their section caused so much pain that I wished that I had operated under general anæsthesia. The reason for the failure of infiltration to reach the larger nerve-trunks, I think, is undoubtedly the fact that the filaments contained in them are so protected by their sheaths that the fluid does not reach them unless the needle has been actually introduced into the sheath, a procedure which we cannot perform with accuracy unless the nerve is freely exposed in the dissection, and which even then is apt to be attended with pain. Operations upon the median line of the body are especially adapted to deep dissections under local anæsthesia, for the reason that no large nerve-fibres are met with, and we have to do only with the terminal filaments which meet in the middle line.

This applies to such operations as median laparotomy, perineor-

rhaphy, and the like. Such operations, as will be explained more in detail below, I would not advocate performing in general under local anæsthesia; but in special cases and emergencies, when a general anæsthetic is inadvisable, they may be performed with considerable facility and freedom from pain under local anæsthesia.

A brief classification of operations, according to the facility with which they may be performed under local anæsthesia, with remarks explanatory of the reasons, will perhaps enable us to appreciate the limits of the applicability of local anæsthesia better than any other method. In Class 1 I would place those operations in which local anæsthesia gives in general perfectly satisfactory results, and in which I would rarely employ a general anæsthetic except for special reasons, such as idiosyncrasy of the patient or the desire on the part of the patient not to be conscious that he is being operated upon. Such operations are the removal of subcutaneous tumors in non-inflamed tissue, particularly in the scalp, face, back of the neck, or median line of the body. Epitheliomata, either upon the face or anywhere upon the body, may be easily and satisfactorily removed under local anæsthesia. Large tumors of this character may often be satisfactorily removed. The skin of the back, particularly near the median line, is very easy to render anæsthetic, as it is sparsely provided with sensitive nerve-filaments. Small circumscribed abscesses, especially those formed by the breaking down of tubercular glands, which are so thoroughly softened as not to require extensive curetting, may be opened and drained under cocaine absolutely without pain.

The removal of foreign bodies from the extremities, such as needles in the hand, and operations for ingrowing toe-nail, can be performed under cocaine with perfect ease, and for these I rarely employ general anæsthesia. Felons of the fingers may be done under cocaine if the inflammation has not extended beyond the second joint, so that the infiltration can be begun in healthy tissue. All the tissues of the fingers and toes may be infiltrated, including skin, subcutaneous tissue, tendons, and periosteum, so that the bone may be amputated without pain. Where bone operations are to be performed, it is frequently wiser to employ a general anæsthetic, as the crunching of bone-forceps and sawing of bone is disagreeable to many patients, even though unattended with pain.

In the removal of pieces of tumors for diagnoses, local anæsthesia has an important place. In general, with regard to operating upon the skin and subcutaneous tissues, it will be found that where the skin and underlying tissues are soft and not stiffened by callous or

tough fascia, infiltration is easy. This is true of the skin over nearly the whole of the body.

In the palms of the hands and soles of the feet, the toughness of the skin and its adhesion to the strong underlying fascia render infiltration difficult, owing to the force required to send the fluid into the tissues, and also the impossibility of judging just how far it has gone.

In Class 2 I would place a class of cases in which local anæsthesia, although not giving ideal results, is often of advantage. Here comes moderately extensive abscesses, where curetting is not required. Such abscesses may be infiltrated and incised absolutely without pain; but the dilatation of the wound and insertion of the drainage-tube are painful, owing to the stretching of the inflamed tissues beyond the anæsthetized area.

In Class 3, operations in which local anæsthesia should not in general be employed, I should place operations upon extensive inflammatory processes, which it is impossible to infiltrate and operate upon without pain, as the tissues are already so distended with the serum as the result of inflammation that further infiltration is impossible. The cocaine cannot be driven in without first driving out the serum from the already distended lymph-space, a process which will often be attended with as much pain as operations without anæsthesia.

In general, the only operations which can be performed with perfect local anæsthesia on inflamed tissues are those in which it is possible to surround the inflamed area by an infiltration in healthy tissues cutting off its nerve-supply. Extensive dissections of the inguinal glands for adenitis have been reported by Bransford Lewis and other authors, as performed under local anæsthesia; but in my own experience, no matter how careful the technique, I have found local anæsthesia unsatisfactory in these cases. In this class of operations, which should not be attempted under local anæsthesia, —that is, in the majority of instances,—should be placed major operations in general, and those which require consultation during the operation between the operator and his assistants or consultants, a participation in which might be demoralizing to the patient. Operations involving exposure and disagreeable manipulations about the genitals or rectum are usually, in sensitive persons, best performed under general anæsthesia. In deep dissections around important blood-vessels and nerves it is usually advisable to employ a general anæsthesia.

As to abdominal operations, the necessity for muscular relaxa-

tion, and the extent and formidable nature of the manipulation required, render them unsuitable for local anæsthesia. In operations for rupture of adhesions about ankylosed joints and other procedures requiring stretching and straining of adhesions, local anæsthesia is of no benefit. In delicate plastic operations, such as those about the eyelids, the infiltration anæsthesia so distorts the flaps as to increase the difficulties of the operation, and therefore it is a rule better to resort to a general anæsthetic.

In the above list of operations in which local anæsthesia is not in general advised are included so large a number of the major operations of surgery, that we may well inquire whether local anæsthesia has any field among operations of a more formidable character. The answer is, that in certain cases of a major character, some of these among the more important surgical emergencies, local anæsthesia has proved of the greatest value.

These are operations which we often have to perform under a vital indication, and in which the employment of a general anæsthetic adds greatly to the danger of losing the patient. In these cases we can often save our patient a vast amount of suffering by the use of local anæsthesia.

The operation of tracheotomy, where required for stenosis of the larynx, may be easily and painlessly performed under cocaine, and the obvious dangers of a general anæsthetic avoided. The dangers of general anæsthesia in operations for neglected strangulated hernia in patients the subjects of chronic bronchitis are well known. The operation for the relief of strangulated hernia may be easily and satisfactorily performed under local anæsthesia. I was recently called upon to operate upon a recently strangulated femoral hernia as large as a fœtal head at term in a woman of sixty years with a weak heart, beating at the rate of 160, who also had a chronic bronchitis. Under local anæsthesia I was able with ease not only to open the sac, incise the constricting ring, reduce the two and a half feet of distended intestine, with the cæcum and appendix, which were contained in the hernia, but to suture the ring, so that I hope a radical cure has been effected. The operation was performed two weeks ago, and the woman's recovery has been uneventful. I felt that general anæsthesia would have added very greatly to the danger of this operation, which was performed with so little pain to the patient that she thanked me at the close of the proceeding for hurting her so little. Intestines can be handled or even incised without pain even without an anæsthetic. I have anæsthetized the abdomen of an old man, the state of whose kidneys would not allow of general

anæsthesia, for Dr. H. W. Cushing, who performed a suprapubic cystotomy, and have seen Dr. Gerster perform a gastrotomy for cancer of the œsophagus under cocaine in a patient too weak for general anæsthesia, and immediately feed the patient through the opening before he left the operating-table.

The chest may be drained under local anæsthesia in empyema, in cases where the pressure of the fluid on the heart made general anæsthesia inadvisable.

In view of these facts, although local anæsthesia is not in general advisable for major operations, there is a certain class in which it is not only advisable, but demanded, and its employment in preference to a general anæsthetic may save the patient's life.

One word with regard to the employment of local anæsthesia in neurasthenic patients. There is a certain class of patients, mostly neurotic women, who desire not only not to feel the pain of the knife, but have such a horror of surgery that the mere knowledge that they are being cut, even though the cutting be painless, will cause them to struggle, protest, and make outcry. After making a tremendous outcry and greatly disconcerting the surgeon, these patients will often tell him that they felt no pain, but the consciousness that they were being operated upon, and the fear that they would be hurt, were so dreadful that they wished they had taken ether. In patients of this sort it is wise to employ a general anæsthetic.

DISCUSSION.

Dr. Potter.—I wish that our essayist would try to use cocaine in some of the cases in which we try to use it. We go into the tough mucous membrane, and underneath that is the periosteum, and every one who has tried it knows that we get into a very hard, unyielding field of operation. Not only that, but when we get an anæsthetized area, we do not always operate there, but at a point somewhat removed, as in the extraction of teeth. Can we reach the nerve-filaments encased in their bony walls? Some of us have done it with success; but I think no one can claim that he always has success. I remember, in 1885, my first use of cocaine by hypodermic injection for the extraction of a tooth. I injected on the outer and inner side of a lower molar root, using a five-per-cent. solution, and putting in at least a minim on each side, and the operation was quite satisfactory. I have used the drug more or less ever since, and it has been my experience that in order to get a very good result in extracting a tooth I need to use a four- or five-per-cent. solution, and put in about one-tenth of a grain. I have searched the authorities

very thoroughly to see what is regarded as a therapeutic dose, and I find that one-eighth of a grain is about the maximum for use in regions about the head. It seems to me that in extraction it is necessary to put in about one-tenth of a grain in order to get a satisfactory result. I have had one unfavorable case in the use of this drug, which put me on my guard. We need to have the necessity of care in its use impressed upon us. If we use the very dilute solutions which have been spoken of here to-night, we are not likely to obtain systemic effects; but in my experience the stronger solutions—four and five per cent.—are better for our purpose.

Dr. Lund.—I should like to say that I think the region around the teeth and gums would be as unsatisfactory a place to work in as could be found. In other parts of the body one can use of the 1 to 500 solution twenty-five syringefuls, of twenty minims each, in an extensive operation without causing the absorption of a dangerous amount, since a good deal of the injected fluid runs out when the tumors are cut and retracted. Of a 1 to 1000 solution, fifty syringefuls could be used without unduly increasing the amount of cocaine. In the removal of tumors, etc., where there is always more or less extensive dissection, these weak solutions are effectual and really safe. But as to getting inside of the bony channels in the mouth, I should think it would be extremely difficult and uncertain.

Dr. Piper.—Will the essayist please tell us how the tissue recovers in those cases where many injections have been made, and what condition it is in after being treated in that way?

Dr. Lund.—The fluid is gradually absorbed. It does not interfere with the healing, provided you inject an aseptic fluid. Naturally, if you inject a septic fluid, it suppurates; but if a perfectly clean solution is used, the œdema will disappear after twenty-four hours, or, at least, be displaced by a slight inflammatory swelling without redness, which quickly subsides and does not interfere with healing.

Dr. Piper.—I used cocaine in 1885, when it was first introduced into this country, and I have continued its use up to the present time, having used a hydrochlorate solution of from twenty to four per cent. The twenty-per-cent. solution was used on the theory that if a very small amount of a strong solution be injected, a better local anæsthesia would be obtained without alarming systemic symptoms.

The local effect was good, so also was the systemic. I then started to use a ten-per-cent. solution, and the results were very

good. I think perhaps I used this solution about five hundred times, without an accident or alarming symptom. Now, however, every patient submits himself to an examination in regard to his physical condition before cocaine is used. Thus, having full knowledge of what may be done and what may be expected, we are prepared to meet any serious obstacle to the success of the operation. One case taught me to take this precaution. The experience of accidents caused by the use of this drug by responsible men, as reported in the leading journals, led me to look about for some other alkaloid to take the place of cocaine hydrochlorate. Stumbling on an article by Dr. Arthur P. Chadbourne on tropacocaine, published in a London journal, I became interested, and immediately procured a few grains, and cautiously began with its use, having the first injection made upon myself for the removal of a small bunch on the side of the face. The operation was painless, without the alarming symptoms which were presented from an injection of hydrochlorate of cocaine some twelve months previous. This was encouraging. I began to use it on patients furnished by a physician, who was in attendance to treat any alarming symptoms that might present themselves. After using tropacocaine some six months, I made an appointment with a surgeon to remove an epithelioma from the face of a patient to whom he did not care to give ether on account of the heart. I suggested the use of tropacocaine. He said, "Very well; if you will take care of the injections, I will take care of the patient." Two grains of the alkaloid were put under the skin before the operation was complete, and, much to our surprise and gratification, not a single symptom from the drug presented itself. While I cannot get the result from tropacocaine that may be secured from cocaine, I prefer to use it because it is much less toxic, and the local anæsthesia very good. I now use tropacocaine more often than I do cocaine.

Dr. Lund.—What strength of solution do you use of the tropacocaine?

Dr. Piper.—Three per cent. It is more expensive than cocaine.

Dr. Lund.—I have tried it only twice, using only a four-per-cent. solution, and I did not get a good result in either instance. The operations in both cases were in inflammatory processes under the lip, and I was discouraged by the failure to produce anæsthesia.

Dr. E. C. Briggs.—I want to emphasize what the essayist has called particular attention to, and that is the obtaining of a wheal. After reading Schleich's paper several years ago, I appreciated that point, and I always aim to produce that wheal, and I find, if I get

it, the success of the operation is assured. It was with that in mind that the preparation which is put up by Dr. Kitchen, with a petroleum base, caught my fancy, and I have used it now for two or three years. As would suggest itself to you, this petroleum base has the effect of confining the preparation about the point of injection. I find the greatest difficulty is to prevent the effect from going off into the surrounding tissues. Dr. Potter says that he has had difficulty in obtaining the effect in dense tissue. That is just where I can do the best with it. In the softer parts—say, for instance, where the cheek is reflected upon the alveolus—you will find that the cocaine disappears immediately into the loose connective tissues, and you get no wheal, no local anæsthesia, and there is danger of getting constitutional effects. I use cocaine in extracting teeth, injecting next the margin of the gum, producing a wheal; then I make another injection just inside the limits of that wheal, and then another, and by making three injections on each side the tooth can be extracted without pain. Then by running the syringe down into the socket and injecting as near to the apex as you can get, the socket of the tooth can be bored out, for the purpose of replantation or transplantation, and the whole operation can be done without any untoward effect. The day before Thanksgiving I extracted a tooth which was badly affected with pyorrhœa. Although the tooth was loose, it came very hard; and yet in that case the extraction was done painlessly. Then I injected into the root socket and cut out the soft tissue that droops down; unless you do that the tooth does not seem to get down to a good base. Then, as I put on the splint, I remarked to the lady that I was sorry she would not be able to enjoy her Thanksgiving dinner, fearing that her mouth would be sore and she would be unable to use the tooth. When I next saw her she said she was better able to eat on that tooth than at any time previous to the operation.

The drug "eucaine" has been receiving considerable attention lately. I wish to say that, coincident with all the cases in which I have used eucaine, I got indurated spots that did not disappear for a long time. Of course, I have sometimes observed this effect after using cocaine, but in the eucaine it was universal, so I went back to cocaine. I have never had any very serious results with cocaine, except perhaps in the case of a gentleman who apparently died while I was injecting cocaine into the tooth to take out the pulp. I was much alarmed, and injected quantities of brandy, and finally succeeded in reviving him. I often thought of that case with feel-

ings of perturbation when I was using cocaine; at the same time there was always a mental reservation as to the cause of the accident. I did not feel that cocaine was the direct cause, but rather that it was shock. Within a week the same gentleman was in my chair. He had a sensitive cavity, and I was about to use the cataphoric apparatus to benumb the sensitive dentine, but he had no sooner taken the cold, clammy electrode in his left hand than he collapsed. He fell back, turned gray, and presented an entirely different appearance from an ordinary faint. It was an extreme case of shock, and I do not know how to account for it unless he has a very bad tobacco heart.

Dr. Eames.—In regard to syncope, I believe that many cases are due to the fact that the patient has eaten nothing for several hours previous to the appointment. Only yesterday I had a case which required some probing in a fistula; it was entirely painless; but the patient immediately fainted. It was about twelve o'clock, and after the patient had recovered I suggested that she should then take lunch, which she did, after which I was able to probe the fistula thoroughly and cut the surrounding tissues without causing syncope in the slightest degree. I have had this experience repeatedly, so that I now do minor operations, not requiring a general anæsthetic, soon after a meal rather than before one.

In regard to the injection of cocaine, I use a syringe having a solid piston, the whole being made of metal, so that it may be boiled or cleansed in any way we wish. For the extraction of a tooth, I believe that it is better to make several small injections on each side rather than to press the needle in deeply and inject the whole quantity at one insertion. By this means we get the "infiltration of tissue" which is desired. With reference to eucaine, I consider it more dangerous than cocaine. I believe that those who declare it to be without danger, do so without knowledge. I will admit that it may be used to a great extent without serious results. It takes very extensive experimentation to arrive at the relative toxic property of such a drug, and we have not had that yet. We know that chloroform may be used hundreds and hundreds of times without fatal results, and yet we know that serious results do occur sufficiently often to influence many of us to abstain from the use of this agent altogether.

Dr. Brackett.—I wish to corroborate out of my experience the remarks of the speaker who has just taken his seat in regard to the toxic effects of cocaine solutions being more noticeable in persons who are in need of refreshments. Some of the most unsatisfactory

experiences that I have had have been with patients who have come in at an early hour in the morning, having passed a sleepless night, perhaps an interval of several days and nights without sleep, and in many cases having taken but little food. They come to my office in a state of general exhaustion. Patients in that condition are just the ones for whom to be cautious about using cocaine.

I began using the drug as early as it was to be had, have continued its use ever since, and I have felt all along very earnestly that the practitioner who does not avail himself of aid of this kind must be subjecting his patients to a great deal of unnecessary suffering. At first I used strong solutions in considerable quantity. From experience I learned that weak solutions are proportionately more effectual than strong solutions, while, of course, a great deal safer. One of the points which has been brought to my mind this evening is that much more attenuated solutions than those which I have used are effectual. I shall go back to my office and prepare solutions of such strengths as the essayist has indicated and use them with confident expectation of safe and satisfactory results. I feel under especial obligations to the essayist for the information which he has given us.

Early in the use of this drug, Dr. J. W. Smith, who is not now living, put in practice an idea which I believe was original with himself, and which I have previously mentioned before this society. That was the adding to the solution of a very minute proportion of carbolic acid, which seems to make the cocaine solution less readily absorbable, less likely to produce constitutional effects. From 1889 or 1890 down to the present time it has always been my practice to add a drop of the deliquesced solution of carbolic acid to cocaine solutions, and it is my belief that on account of so doing I have had fewer cases of systemic disturbance. I believe there is another advantage in doing this. Although I always prepare very limited quantities of cocaine solutions at a time, I have felt that the carbolic acid was an additional security against its contracting any septic contamination, and I should, therefore, feel safer in using such preparation in case it had been standing a few days.

With regard to the structure of the tissue into which injections are made, it seems to me that the dentist has some advantages over the operator upon other tissues than the gums. In firm, compact tissues much better results are to be attained than in very loose tissues. An efficient syringe, by means of which considerable force can be supplied, is also important.

Dr. Williams.—The essayist made one remark bearing upon an

idea with which I have had some experience. He stated that in a case on which he operated, which resulted in collapse, he "hit him over the heart" and started the circulation. I simply wanted to say that at one time chloroform was substituted for ether, and after that chloric ether was substituted for chloroform, and partly from want of knowledge of the proper application of these drugs many accidents resulted. We always gave the anæsthetic gradually, and when cases of apparent collapse came we produced a bellows motion of the chest and lungs by compressing and relaxing the ribs, and by thus forcing air to the heart made the heart act. Fortunately, we never had a fatal case. I had one patient for whom a few roots were to be taken out, and he suggested that his brother, then a young physician, should administer the anæsthetic. I was very glad to have him, of course, and he came and gave chloric ether,—gave it to him till he thought he was insensible. I then took out the roots, and just as I took out the last one the patient fell back, his face became livid, his lips purple, and his eyes glazed, and his breath ceased. The doctor's face turned as pale as the patient's, and he called for ammonia or some electrical apparatus. I told him there was no time to look for machinery, and that we must keep up the motion of the chest to get the patient breathing. I began at once to press with a steady, intermittent, regular motion. The elastic expansion of the chest drew the air into the lungs and stimulated the heart to keep up its action. I have frequently thought since that if we had lost half a minute in looking for apparatus, it would have been a fatal case. So I believe that in cases of collapse, if you keep up the action of the lungs, you are pretty sure to keep up the action of the heart. Dr. Eames and Dr. Brackett spoke of their unsatisfactory experience with patients early in the morning, and I have had several of those cases. Early in my practice I learned that the morning is the time when the nervous system is most readily exhausted or depressed, and is more easily affected by the use of narcotics or stimulants, and the system is more easily drained by effort, mental or physical, than later in the day. Old Dr. Homans said that he often noticed that a dose of opium early in the morning would have a greater effect than after eleven or twelve o'clock in the day. I remember seeing statistics on the subject of delirium tremens by a French physician, and he stated that all of his cases were morning drinkers,—they took the stimulant at the time when the system was most easily upset. In view of these facts, I have always felt that it was important to save the morning strength. Speaking of that to a lady not

long ago, she concurred in my idea, and told me of what she thought was a mistake on the part of one old doctor. He used to get his daughters up in the morning to walk around the Common, and not one of them has any nerves now she said. I have also noticed this in studying the obtunding of sensitive dentine. I have found that patients complained more in the early hours of the day than later in the day, so that I came to adopt the rule to make appointments for what promised to be sensitive cases as late as convenient in the day and not in the morning.

Dr. Lund.—I have never used cocaine on the same person more than once on a given day. I think the rule that Dr. Potter spoke of—not using more than one-eighth of a grain—is a very safe one. The face is near the nervous centres, and the effects of the cocaine would be more quickly felt. It would be safe in other parts of the body to go up to half a grain, if you do not do it more than once. I should never go beyond half a grain except in long operations, when a good deal of the cocaine, first injected, has run out through the incision before the remainder is put in.

Dr. Piper.—On one occasion I made an injection of a ten-per-cent. solution for the purpose of drilling through the gum for amputation of the end of a root. The operation was successful, without symptoms of cocaine-poisoning. The next day I had occasion to make another injection for the same patient, using the same amount of the solution that I did the day before, and, much to my surprise, certain symptoms presented themselves which have been attributed to a toxic dose of cocaine hydrochlorate. My belief that cocaine may be accumulative is based upon this one case.

Dr. Eames.—I should say that cocaine is not cumulative. For the past six years I have used cocaine many times daily, and have carefully observed its effects, and my observations lead me to believe my statement to be correct.

WILLIAM H. POTTER, D.M.D.,
Editor American Academy of Dental Science.

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